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WEGE UND ZIELE DER PSYCHOLOGIE IN DEUTSCHLAND

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Ein Hauptmerkmal der Psychologie im neuen deutschen Reich besteht darin, dass sie nicht mehr eine isolierte, von allen anderen Wissensgebieten abgetrennte Spezialangelegenheit ist. Ohne ihre Exaktheit und ihren Charakter als besondere Wissenschaft im geringsten preiszugeben, trat sie in eine enge Arbeitsgemeinschaft mit den naturwissenschaftlich-medizinischen und ebenso mit den geisteswissenschaftlichen Nachbarfächern ein. Im 19. Jahrhundert hatte sich überall ein verhängnisvoller Gegensatz ausgebildet zwischen der Medizin und Naturwissenschaft auf der einen, der Geisteswissenschaft auf der anderen Seite. Ein unhaltbarer Zustand, wenn man bedenkt, dass die Wissenschaften am Aufbau einer *einheitlichen Kultur* wesentlich mitzuarbeiten haben. Gerade diese Forderung stellt an die Lehr- und Forschungsstätten unser neuer Staat. Er dient aber damit nicht nur dem Volke und dem Aufbau einer einheitlichen Kultur, sondern zugleich der reinen Wissenschaft und der Wahrheit selbst, mit deren Grundsätzen es ja unverträglich ist, dass die verschiedenen Wissenschaften untereinander über dieselben Gegenstände dauernd im Streite liegen und Widersprechendes lehren. Der Streit der Fachgebiete kann natürlich nicht durch ein von aussen kommendes Diktat behoben werden, sondern nur durch eindringende Forschung, und zwar gerade in den Fächern, welche zwischen den kämpfenden Wissenschaften liegen und dank der Eigentümlichkeiten ihres Gegenstandes eine Art von Brücke zwischen ihnen darstellen. Keinem anderen Fache vielleicht fällt im Hinblick auf dieses Ziel, das ein wissenschaftliches, kulturelles und staatliches zugleich ist, eine so grosse Aufgabe, aber auch

so schwere Verantwortung zu wie der Psychologie; denn sie ist mit besonders zahlreichen und besonders gegensätzlichen Gebieten von Wissenschaft und Leben verknüpft. Da ihr Gegenstand, das Seelenleben, sowohl der natürlichen wie der geistigen Welt angehört, so gelangt in ihrem Gebiet notwendig auch der härteste Gegensatz im Bereiche der Wissenschaft, derjenige zwischen Naturwissenschaft und Geisteswissenschaft, zum Austrag und, wie sich jetzt in jedem Jahre deutlicher zeigt, zur Versöhnung.

Unter den verschiedenen Formen der Arbeitskameradschaft, die sich immer zunehmend angebahnt haben, ist keine andere so wichtig und in solchem Ausmass verwirklicht, wie diejenige zwischen Psychologie und Medizin auf der einen, Psychologie und Kulturphilosophie auf der anderen Seite. Die ersten, mehr auf genialer Intuition als schon auf eigentlicher wissenschaltlicher Forschung beruhenden Anfänge dieses *wissenschaftlichen Dreibundes* gehen auf Nietzsche zurück, der heute mit Recht als ein früher Wegbereiter unserer neuen Kulturgestaltung in Deutschland angesehen wird.

Das 19. Jahrhundert hatte mit Hilfe der Wissenschaften von der unorganischen Natur und der Technik im weitesten Sinne eine "positivistische" Kultur der toten Sachen und Sachwerte begründet und zu grosser Höhe entwickelt. Die Philosophie, mit der sich die historischen Geisteswissenschaften gegen den "Positivismus" verbündeten, stellte daneben ein Bildungssystem der reinen, wirklichkeitsfernen Ideen; sie vermied geflissentlich den Weg der Erfahrungserkenntnis und legte im Gegenteil immer zunehmend Wert auf ihren "Apriorismus" und auf ihre "Reinheit," d.h. eben Reinheit von Erfahrung. Die Wissenschaft in ihrer Gesamtheit zerfiel auf diese Weise in *zwei Hälften*, die keine Berührung mehr miteinander hatten und sich an den Stätten der Wissenschaft wie zwei feindliche Heerlager gegenüberstanden. Der Riss in der Wissenschaft und im Bildungssystem erschwerte die Einigung des Volkes. Nur die sogenannten "Gebildeten" konnten den Versuch machen, dem seelisch immer unbefriedigender werdenden Dasein zu entfliehen und sich zu der "Höhe" einer wirklichkeitsfernen, reinen Ideenwelt emporzuschwingen. Die einfachen Menschen dagegen, also die weitaus meisten im Volke, blieben, z.B. als Industriearbeiter, der toten Sachkultur verhaftet und entbehrten darum, was noch verhängnisvoller als Armut ist, aller seelischen Lebensgüter.

Neben diesem Kultur- und Wissenschaftssystem der *toten Sachen* und der *reinen Ideen*, das somit in zwei getrennte Hälften gespalten war, kam gerade der *wichtigste* Bereich zu kurz, der Bereich des *Lebendigen*,

der zwischen der unorganischen Natur und der hochgeistigen Ideenwelt gleichsam in der Mitte liegt. Weil sich niemand um die natürlichen Lebensgesetze kümmerte, darum erkrankte und degenerierte das Leben in weitem Umfang. Nietzsches Darstellung der Kultur der letzten Jahrhunderte ist, und zwar ganz bewusst, die Schilderung einer Kultur*erkrankung*. Es ist nun ein Ziel unseres neuen Staates, unsere deutsche Kultur auf die natürlichen Lebensgesetze zu begründen, und zwar auf diejenigen normalen, gesunden Lebens. Die Grundabsicht unserer heutigen deutschen Bewegung im Kulturbereich ist, wie die eines Arztes—gleichsam eines "Arztes der Kultur"—auf *Heilung* gerichtet. Sie hat nicht das wild verzerrte Gesicht des Kriegsgottes, sondern das menschenfreundliche Antlitz der Asklepios. Sie ist darum im tiefsten Sinne *sozial*. Denn indem wir, im Unterschied zu der Kultur der materiellen Werte und hochgeistigen Ideen, eine Kultur des *Lebendigen* errichten und das gesamte Dasein auf die natürlichen Lebensgesetze begründen, schaffen wir eine *gemeinsame* Plattform, auf der sich die einfachen Menschen und die sogenannten Gebildeten zusammenfinden können; denn in allen Gliedern eines Volkes ist *dieselbe Form des Lebens*.

In dem Dienst dieser Ziele steht auch die grosse Bewegung zur *Typenlehre*, die in der deutschen Psychologie schon viele Jahre vor dem Umbruch eingesetzt hatte, etwa gleichzeitig mit dem Aufkommen der Rassenkunde. Der Unterschied zwischen psychologischer Typenlehre und Rassenlehre besteht darin, dass die Rassenlehre *biologisch-psychologisch* und *zugleich historisch* vorgeht, indem sie die heute vorhandenen Formen und zugleich ihre *Herkunft* zu erforschen unternimmt, dass die Typenlehre dagegen *ausschliesslich psychologisch und biologisch verfährt*. Sie beschränkt sich darauf, die Formen des heute vorhandenen lebendigen Menschentums zu untersuchen, indem sie die Frage nach der historischen Herkunft dieser Formen der *Geschichte* und *Vorgeschichte* zur Bearbeitung nach *ihren* Methoden überlässt. Trotz oder vielmehr wegen dieser Arbeitsteilung besteht dauernd Wechselwirkung und Gedankenaustausch zwischen psychologischer Typenlehre und Rassenforschung.

Gemäss ihrer engen Arbeitskameradschaft mit der *Medizin* erforscht die Typenlehre die menschlichen Grundformen hauptsächlich auch daraufhin, inwieweit sie Ausdruck eines *gesunden*, starken und aufsteigenden oder aber eines kranken, entarteten und niedergehenden Lebens sind. Sie sucht damit zugleich der zuerst von Nietzsche in der Kulturphilosophie betonten Forderung zu entsprechen, dass man

auch in der Kultur, ähnlich wie beim Einzelleben, *Krankes* in Heilung überführen müsse. Die Entstehungsweise der heute weit verbreiteten kranken und entarteten menschlichen Grundformen zu erforschen, betrachtet darum unsere Typenlehre als eine ihrer Hauptaufgaben.

Obwohl die Bestrebungen der Typenlehre von *verschiedenen* Ausgangspunkten herkommen, bahnt sich jetzt zwischen den verschiedenen Typologien eine zunehmende *Einigung* an. Ich kann darum bei der Erläuterung der praktischen Ergebnisse der Typenlehre von der von mir selbst vertretenen *Integrationstypologie* ausgehen. Als ein Kennzeichen der menschlichen Persönlichkeit—wie überhaupt des Lebens im Gegensatz zur unorganischen Natur—ist seit längerer Zeit ihre *Einheit* und *Ganzheit* hervorgehoben worden. Aber es genügt nicht, immer nur diese "Ganzheit" in allgemeiner Form zu betonen. Vielmehr müssen *verschiedene Formen* dieser Ganzheit unterschieden werden, und ausserdem gibt es Typen der menschlichen Person, denen diese Ganzheit entweder überhaupt oder in irgend einem bestimmten Sinne *fehlt*. Auf diese *Formen* der Einheit und Ganzheit, sowie auf ihr *Fehlen*, richtet die Integrationstypologie ihr Augenmerk, wie schon ihr Name zum Ausdruck bringt. Sie fasst somit *ganz allgemeine Strukturen* ins Auge, die sich in den allerverschiedensten —elementaren und höheren— Schichten der psychophysischen und psychischen Persönlichkeit verwirklichen können. In der Tat konnte die Integrationstypologie beweisen, dass solche allgemeine Strukturen durch alle Schichten der Persönlichkeit hindurchziehen. Das gilt jedenfalls von den einigermassen ausgeprägten Typen. Hieraus ergibt sich die wichtige Tatsache, dass die menschlichen Grundfomen *biologisch* verankert und nicht allein durch *hochgeistige Ideenwelten* geformt sind, wie man es sich in der Kulturphilsophie des 19. Jahrhunderts vorzustellen pflegte. Verhielte es sich in dieser Weise, dann könnte nicht die Grundstruktur der menschlichen Persönlichkeit bis in die elementaren psychophysischen Schichten hinab nachweisbar sein. Schon hierdurch tritt die Typenforschung den Grundanschauungen des verklingenden Kultursystems der reinen Ideen entgegen, und sie trägt umgekehrt auf diesem Wege bei zum Aufbau einer *Kultur des Lebendigen*.

Das wird noch sehr viel deutlicher, wenn man die menschlichen Grundformen einzeln ins Auge fasst. Eine sehr verbreitete dieser Grundformen ist gekennzeichnet durch die *Auflockerung* und, bei stärkerer Ausprägung, *Auflösung* und *Zersetzung* ihrer Strukturen, und zwar in sämtlichen Funktionsschichten, vom Höhergeistigen bis zum Elementarseelischen, bis zu Empfindung, Wahrnehmung und selbst

bis ins Physiologische hinab. Wir nannten diesen Typus anfangs S-Typus, wegen des bei ihm besonders häufigen Stigmas der Synästhesien. Als unser Beobachtungsmaterial anwuchs, ergab sich jedoch, dass der Auflösungs- und Auflockerungstypus auch ohne das Vorhandensein von Synästhesien vorkommt. Die Synästhesien spielen hier also lediglich die Rolle eines Stigmas in dem Sinne, in dem P. Janet diesen Begriff bestimmt hat.

Der *durchgehende Grundzug* dieses Typus ist die *Labilität*, die *Aufgelockertheit* und *Aufgelöstheit* aller psychischen und psychophysischen Funktionen. Hierauf beruht auch sein Mangel an Wirklichkeitskontakt. Diese Ergebnisse stehen in einer nahen Beziehuung zu P. Janet's Lehre von der besonderen Schwierigkeit des Kontaktes mit der Wirklichkeit. Dieser Lehre muss jedoch eine *typologische* Ergänzung hinzugefügt werden: Der Kontakt mit der Wirklichkeit ist innerhalb der heutigen Kulturmenschheit nur darum eine so besonders schwierige Funktion, weil der *Auflösungstypus* in ihr so weit verbreitet ist. Es gibt *andere*, zum Auflösungstypus gegensätzliche Formen des menschlichen Seins, für die der Kontakt mit der Wirklichkeit etwas Selbstverständliches, Natürliches und darum auch *Leichtes* ist.

Ausser dem erwähnten *einfachen* Auflösungstypus (S_1) gibt es noch einen höheren (S_2). Er besitzt in den Elementarschichten der psychophysischen Person, in Empfindung, Wahrnehmungs-, Vorstellungs- und Phantasiewelt dieselbe Labilität und Haltlosigkeit wie S_1. S_2 ist nur eine höherdifferenzierte Weiterbildung des S_1-Typus, der in S_2 immer eingeschlossen bleibt: $S_2 = S_2 (S_1)$. Dieser *höhere* S-Typus unterscheidet sich von dem *einfachen* dadurch, dass er gegenüber dessen Labilität und Haltlosgkeit eine *Kompensation* bestizt, ähnlich wie es bei einem "kompensierten" Herzfehler der Fall ist. Die Kompensation besteht in einem meist stark entwickelten *Intellekt* von besonderer Art: einem Intellekt, der von allen anderen Schichten der psychophysischen Persönlichkeit *abgelöst* ist: von Gefühl und Instinkt, von Trieb- und Willensleben, von allem, was wir in dem Begriff "Charakter" zusammenfassen, ja unter Umständen sogar von Empfindung, Wahrnehmung, Anschauung und Erfahrung.

Wegen der Labilität und mangelnden Überzeugungskraft seiner Empfindungen, Wahrnehmungen, Denkakte kennt der ausgeprägte Auflösungstypus *keine feste Gegebenheit* innerhalb der Aussenwelt. Infolge der Labilität und Unsicherheit seiner Instinkte, Triebe und Willensregungen hat er auch in seinem Innenleben keine fest gegebenen Richtlinien. Als Ersatz der *gegebenen* Welt *konstruiert* er rein gedanklich

eine Welt aus seinem "abgelösten" Intellekt heraus, ohne Kontakt mit den Gegebenheiten in Aussenwelt und Innenwelt. Gegen seine Auswirkung in der Wissenschaft und die daher rührende "Scholastik" hat die deutsche Psychologie seit langem angekämpft. Neben dem Verfasser tat dies früh und mit starkem Einsatz namentlich H. Schole.

Kulturbewegungen von grossem Ausmass beruhen immer auf einer instinktiven Psychologie. Wenn diese *instinktive* Einsicht richtig ist, dann muss sie mit den Ergebnissen der *wissenschaftlichen* Psychologie und der wissenschaftlichen Erkenntnis zusammenstimmen. Unsere Bewegung wendet sich als ein "Arzt der deutschen Kultur" mit heilenden Händen gegen alle Erscheinungen von *Auflösung und Zersetzung* im Bereiche unseres Daseins. Der Typologie drängt sich die Notwendigkeit eines solchen Vorgehens mit ihren *wissenschaftlichen* Erkenntnismitteln auf; denn die weite Verbreitung der Erscheinungen von Auflösung und Zersetzung in unserem Leben rührt eben daher, dass dies alles Lebensäusserungen des ausgeprägten S-Typus sind, und dass dieser, infolge des Zusammenwirkens vieler Ursachen, heute weit verbreitet ist. Unsere Bewegung bekämpft ferner den "*Intellektualismus*." Intellektualismus ist die besondere Form geistiger Betätigung, die dem "abgelösten" Intellekt des S_1-Typus eigentümlich ist, und dieser Typus des Intellektes hat unserer Kultur in den letzten Jahrhunderten immer zunehmend das Gepräge gegeben. Unsere Bewegung bekämpft den Intellektualismus, aber sie befördert den *Geist*, d.h. denjenigen Typus des Intellektes, der mit dem vollen und ganzen Menschentum, mit Empfindung und Wahrnehmung, Gefühl, Wille und Charakter in Verbindung steht, der der Anschauung und Erfahrung bedingungslos aufgeschlossen ist und zugleich aus *innerer Tiefe* schöpft.

Indem wir den S-Typus im Falle nur geringerer Ausprägung als "*Auflockerungstypus*" bezeichnen, bringen wir schon zum Ausdruck, dass er unter Umständen auch sehr wertvolle Eigenschaften besitzen kann. Wenn sich eine solche *leichte* Auflockerung auf eine in ihrem innersten Kern *feste* Persönlichkeit aufsetzt, so verleiht ihr dies unter Umständen ein hohes Mass von geistiger Beweglichkeit, die dann der Ursprung einer besonderen geistigen Schöpferkraft sein kann. Diese *wertvolle* Form der Auflockerung, die auch in zahlreichen anderen Ländern Europas und der übrigen Erdteile häufig sein dürfte, fanden wir besonders in einigen Landesteilen Deutschlands, wo eine Vermischung nicht allzu verschiedener Formen stattgefunden hat, *die der Zeit nach weit zurückliegt*. Dagegen führt die Vermischung *sehr heterogener* Formen, z.B. verschiedener *Rassen*, unausweichlich zu dem höchsten

Grade des S-Typus, dem Auflösungstypus. In dieser Richtung drängt aber überhaupt jede stärkere Vermischung, *wofern sie zeitlich noch naheliegt*. Die durchschnittliche Beschaffenheit der Bevölkerung ist nun derart, dass wir heute für *Verfestigung* der Strukturen Sorge tragen müssen und nicht für noch weitere *Auflockerung*. Denn auf Auflockerung und Auflösung der Strukturen drängen heute so viele Faktoren hin, dass wir hiervon immer noch genug übrig behalten und vielmehr allen Grund haben, für *Verfestigung* der Strukturen zu sorgen, was vor allem Erbhygiene, daneben eine körperlich-seelische Hygiene überhaupt und eine geeignete Erziehung erforderlich macht. Es muss übrigens erwähnt werden, dass es auch einen S-Typus gibt, der keinerlei Merkmale von Degeneration und Entartung darbietet, obwohl er bei fast allen experimentallan Tests ganz ähnlich reagiert wie der lytische S-Typus. Dieser Typus "S-vital" kommt bei uns in Deutschland, wo die S-Merkmale fast immer auf S-lytisch, d.h. den Auflösungstypus, hinweisen, verhältnismässig selten vor, scheint aber in einigen romanischen Ländern—in Frankreich, Italien und namentlich in Spanien—besonders häufig zu sein.

In Deutschland also finden wir den S-Typus durchaus überwiegend in der Ausprägungsform *S-lytisch*. Er entsteht in unserem hiesigen Beobachtungsmaterial in erster Linie und in grösster Reinheit bei heterogener *Rassen- und überhaupt Blutmischung*, namentlich dann, wenn diese zeitlich noch nicht weit zurückliegt; an zweiter Stelle bei chronischen Aufbrauchkrankheiten, vor allem, nach unseren Erfahrungen, bei *Tuberkulose*. Der Umstand, dass sich heterogene Blutmischung, an dem besonders fein reagierenden Indikator der psychophysischen Strukturen gemessen, in derselben Weise wie eine *Krankheit* auswirkt, ist ein Beweis dafür, dass die heterogene Blutmischung nichts Normales und Natürliches sein kann.

Im Gegensatz zu diesem Auflösungstypus stehen die *organischen* oder *integrierten* Grundformen (J-Formen). Ihre Struktur besitzt das Merkmal der *Einheit* und *Ganzheit*. Aber diese ist bei den einzelnen J-Formen von sehr *verschiedener* Art:

J_1 ist der "unbedingt" nach *aussen* integrierte Typus. Er steht mit der sinnlich wahrnehmbaren, namentlich sichtbaren Umwelt in stärkster Verbindung (Kohärenz), und zwar ohne Einschränkung, also "unbedingt." Bei allen organischen, ganzheitlichen Grundformen gibt es ein *zentrales* Funktionsgebiet (sog. "Integrationskern"), von dem alle Tätigkeit ausgeht, das aber auch alle anderen Funktionen in sich aufnimmt und somit die "Endstrecke" ist, in die alle Betätigungen wieder

ausmünden. Dieses zentrale Funktionsgebiet liegt bei J_1 an der *Aussenseite* der Person, dort, wo sie mit der Aussenwelt in Kontakt steht, gleichsam in ihrer "seelischen Haut:" im Bereiche des Schauens, also in dem der Sinne, der sinnlichen Vorstellungen und ausserdem in dem des Ausdrucks. Darum entspricht *künstlerische* Wesensart diesem Typus, oder sie erfordert wenigstens einen Einschlag von ihm. Denn eine jede Kunst stellt Gefühle und Gedanken in *sinnlich fassbaren* Erscheinungen dar.

J_2 ist der *bedingt* nach aussen integrierte Typus. Er hat in seinem Inneren feste beharrliche Kerne, vor allem in Gestalt von Werten und Idealen. Es ist der Typus des Idealisten. Er vermag nur zu denjenigen Inhalten der Umwelt ein inneres Verhältnis zu gewinnen, eine Kohärenz einzugehen, die zu seinen festen Kernen in Beziehung stehen, namentlich zu seinen Idealen. Seine Kohärenz mit der Aussenwelt ist *selektiv*.

J_3, der rein *nach innen* integrierte Typus, ist in seinen ausgeprägten Formen gar nicht mit der sichtbaren Umwelt verknüpft. Sein "Integrationskern" und der Antrieb seines Handelns liegt in den ganz tiefen, instinktiven und vorbewussten Schichten des Gefühls, Instinkts, Trieb- und Willenslebens. Er ist im Höchstmass das, was man heute bei uns "blutgebunden" oder "erdgebunden" nennt.

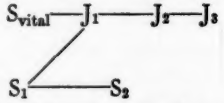

FIG. 1. SCHEMA DER TYPEN

Auf der Linie von J_1 bis J_3 (Fig. 1) nimmt die Aussenintegration, d.h. die Verknüpfung mit der sinnlich wahrnehmbaren, besonders sichtbaren Umwelt fortgesetzt ab, dagegen nimmt auf diesem Wege fortwährend zu die Innenintegration: die Festigkeit der Verknüpfung der Innenweltinhalte untereinander, die "feste innere Linie." J_3 ist der Typus der *festesten* Strukturen, und zwar in allen Persönlichkeitsschichten, von den elementarsten bis zu den höchsten hinauf.

Wegen dieser Strukturfestigkeit ist J_3 das stärkste Gegenteil zu S-lytisch, dem Typus mit den lockersten Strukturen. J_3 findet sich demgemäss in Deutschland vor allem in den reinrassigen, unvermischten, besonders bäuerlichen Gebieten; rassische Unvermischtheit scheint die wichtigste Vorbedingung seines Auftretens zu sein.

Alle diese Typen sind *biologisch*-psychologische Typen. Denn ihre Strukturmerkmale finden sich nicht nur im höchsten, sondern ebenso im

mittelhohen und elementarseelischen Geschehen; sie sind darum auch mittels des psychologischen Experiments erfassbar und lassen sich heute teilweise schon bis ins rein Physiologische hinab verfolgen. Die Existenz der menschlichen Grundformen und ihre ausschlaggebende Wichtigkeit für alle Gebiete des Daseins zeigt, wie notwendig es ist, ihnen Rechnung zu tragen. Hieraus folgt, dass unser Kultursystem nicht, wie im 19. Jahrhundert, auf reine Geisteswissenschaft begründet sein darf, sondern dass es *biologisch-psychologisch* unterbaut sein muss; denn das *wirkliche Menschentum* ist biologisch-psychologisch unterbaut, und das Bild, das der wirklichkeitsferne Spätidealismus vom Menschentum gezeichnet hatte, ist eine Chimäre.

Aus ihrer sichren instinktiven Psychologie heraus hat die deutsche Bewegung schon von Anfang an diesen *biologisch*-psychologischen Gesichtspunkt betont, indem sie die Bedeutung der *Rassenlehre* hervorhob. Obwohl sich psychologische Typologie und Rassenkunde unabhängig voneinander entwickelt haben, sind sie jetzt in das Verhältnis einer kameradschaftlichen Zusammenarbeit eingetreten, durch die auch die Probleme und Ziele der Rassenkunde viele neue Aufklärungen erfahren. Insbesondere werden durch diese Zusammenarbeit von Typologie und Rassenkunde manche Missverständnisse aufgeklärt, die im Ausland hinsichtlich der Rassenkunde immer noch verbreitet sind. Man kann Ziel und Methode der von H. Günther, F. Clauss, E. Fischer u.a. vertretenen Rassenkunde nicht gründlicher missverstehen als es in der Behauptung geschieht, dass hier die unbedingte Überlegenheit einer bestimmten Menschenrasse über alle anderen Rassen behauptet werden solle. Das Ziel, das sich die Rassenkunde stellt, wird am besten klar, wenn man an sie das Bild des *Auflösungstypus* heranhält, das die Typologie herausgearbeitet hat. Dieser Typus S-lytisch entsteht vor allem bei sehr heterogener Vermischung. Die Rassenkunde will dieser heterogenen Vermischung entgegenarbeiten, indem sie unserem Volke die arteigenen und auf der anderen Seite die von seinem Wesen stark abweichenden Formen vor Augen hält; alles dies als eine "kulturärztliche" Massnahme zur Vermeidung der biologisch ungünstigen Wirkungen einer heterogenen Vermischung. Natürlich werden von unseren arteigenen Menschenformen die biologisch und seelisch *hochwertigsten* am stärksten hervorgehoben. Während nämlich der überall hin eindringende und stark vermischte *ostische* Typus, offenbar *wegen* dieser Vermischung, viele Merkmale des Auflösungstypus zeigt und darum seine weite Verbreitung unter biologisch-psychologischen Gesichtspunkten nicht erwünscht sein kann, sind in unserem deutschen Volke der

nordische und *fälische* Typus diejenigen Formen, die von dem in unserer Bewegung bekämpften Auflösungstypus *am allerweitesten* abliegen. Der fälische Typus fällt unter die Grundform J_3. Das Bild, das die Rassenkunde vom nordischen Typus zeichnet, ist überwiegend eine Legierung unserer beiden vorherrschenden deutschen Grundformen J_2 und J_3 (also J_2/J_3). Fälischer und nordischer Typus gehören also in den Bereich der *festesten*, vom Auflösungstypus besonders weit entfernten Grundformen; vermutlich deshalb, weil sie besonders wenig vermischt sind und darum besonders frei von den Bedingungen, die zum Auflösungstypus führen. Die Rassenkunde will somit der *Strukturauflösung*, die Platz gegriffen hatte, entgegenwirken, indem sie auf die Formen mit festester Struktur hinweist. Wenn die Rassenkunde unserem deutschen Volke den nordischen Typus ganz besonders empfiehlt, so wird das Recht hierzu von anderer Seite her auch von der Typenforschung erwiesen. Als Legierung der beiden in unserem deutschen Volke vorwaltenden Grundformen verbindet der nordische Typus die Vorzüge von beiden.

Die Vorzüge des Idealistentypus (J_2) bestehen in seinem Schwung, seiner Begeisterungsfähigkeit und Tatkraft. Die Gefahrenzone liegt für ihn darin, dass er, einem hohen und fernen Ziele nachstrebend, den Boden der Wirklichkeit leicht unter den Füssen verliert. Der blut- und erdgebunden J_3-Typus ist ganz fest mit der Wirklichkeit verknüpft, neigt aber, wo er *rein* auftritt, stark zu seelischem *Beharren*. In der Legierung J_2/J_3 wird der dynamische J_2-Typus durch die Statik des J_3-Typus ergänzt, und der statische J_3-Typus durch die Dynamik von J_2. Die beim *reinen* J_2-Typus oft *wirklichkeitsfernen* Ideale werden bei J_2/J_3 zu "blut- und erdgebundenen" Leitbildern. Was bei J_3 nur als zielsicherer Instinkt oder tiefes Gefühl wirkt, wird bei J_2/J_3 zum bewussten Ideal erhoben und ist dann antriebskräftiger und klarer als der blosse Instinkt.

Der "mittelländische" oder "westische" Typus der Rassenkunde fällt unter die Grundform J_1, die in ihrem Gesamtwesen—in exakt fassbarer Weise namentlich im *Farbensinn*—eine Affinität zu *südlichen* Wohngebieten und zu südlichem Menschentum erkennen lässt.

Die Typologie hat die Beschaffenheit des *heute lebenden* Menschentums zum Gegenstand. Dessen *Entstehung* und *Herkunft* erforscht die *Vorgeschichte*. Es kann nicht ausbleiben, dass Typologie und Vorgeschichte auf ihren verschiedenen Wegen einander begegnen. Schon jetzt wird die Einhelligkeit ihrer Ergebnisse erkennbar.

Nach dem Ertrag der typologischen Forschung sind es die Grund-

formen J_1 und J_3, die im deutschen Volke vorwalten und seiner Wesensart vor allem das Gepräge geben. Die Vorgeschichte hat nun festgestellt, dass das Germanentum, und damit auch das Deutschtum, *durch Verschmelzung zweier artverwandter Bevölkerungsbestandteile* entstanden ist, der "Megalithleute" und der "Einzelgräberleute". Die Träger der Megalithkultur werden als eine sesshafte, ackerbautreibende Bevölkerung geschildert, von einer Wesensart, die dem J_3-Typus entsprechen dürfte. Die "Einzelgräberleute" waren ein bewegliches, viel gewandertes Reitervolk, das nach dem Gesamtzeugnis der prähistorischen Funde am ehesten der Grundform J_2 zugerechnet werden muss. Der Drang in die Ferne, im eigentlichen und übertragenen Sinne, der physische und namentlich auch der geistige Wandertrieb, ist noch heute ein Grundzug im Wesen unseres deutschen J_2-Typus. Somit kehren wir, wenn wir in unserer deutschen Bevölkerungspolitik die Legierung J_2/J_3 begünstigen, damit zurück zur Pflege der ursprünglichen Bestandteile unseres Volkes, und wir sorgen auf diesem Wege für dessen artgemässe Reinerhaltung.

Dieselbe Einhelligkeit, wie mit der Vorgeschichte, ist festzustellen zwischen der typologischen Forschung und den psychologischen Anschauungen, die aus den Erfahrungen der *politischen Arbeit* heraus erwachsen sind. Von diesem Ausgangspunkt her griff H. A. Grunsky wieder auf die Lehre Platons zurück, wonach *Seele und Staat* einander entsprechen, und die Staatsform nicht nur eine *beliebige* Äusserung des seelischen Typus—*eine* unter *anderen*—ist, sondern eine *grundwesentliche*, indem die staatliche Ordnung (bzw. Unordnung) das innerseelische Gefüge der Menschen, die den Staat bilden, genau wiederspiegelt. Es ist unzweifelhaft richtig, dass die Psychologie des 19. Jahrhunderts geneigt war, diese Zusammenhänge zu übersehen, da sich der Positivismus die Menschenwelt nach den—heute übrigens veralteten—Modellen der Wissenschaft von der *unorganischen* Natur vorzustellen pflegte, und darum als eine Ansammlung isolierter Atome. Aber die neuere Typenforschung, die sich von diesen positivistischen Vorurteilen nicht nur frei gemacht hat, sondern ihnen bewusst entgegentritt, hat dem grundwesentlichen *Zusammenhang zwischen Individuum und Gemeinschaftsform* schon sehr bald ihre Aufmerksamkeit zugewandt. So stellte insbesondere die Integrationstypologie fest, dass die verschiedenen Formen menschlicher Ganzheit, die den Persönlichkeitsaufbau des Individuums bestimmen, in entsprechender Weise bei den *Gesellungsformen* dieser Typen wiederkehren. Ihre menschlichen Zusammenschlüsse zeigen dieselbe Struktur wie die Ganzheit, zu der ihre individuelle

Persönlichkeit zusammengeschlossen ist. Der Typus, für den die Auflösung der seelischen Strukturen charakteristisch ist, neigt auch zur *Auflösung der sozialen Zusammenschlüsse und* kennt keine *organisch erwachsene menschliche Gemeinschaft*. Wie der höhere Auflösungstypus in sein eigenes Seelenleben nur vom *Verstande* aus Einheit, Ganzheit und Ordnung hineinzubringen vermag, so sind auch seine menschlichen Zusammenschlüsse vom *Verstande* gestiftet: *Gesellschaften*, nicht natürlich und organisch aus Gefühl und Trieb hervorgegangene *Gemeinschaften*. Diese letzteren sind eine Eigentümlichkeit der J-Formen. Dem nach aussen integrierten J_1-Typus muss die Gemeinschaft durch ein *Augenerlebnis* gegeben oder wenigstens durch sichtbare Symbole vertreten sein. Bei J_2 ist sie zusammengeschlossen durch den gemeinsamen Aufblick zu einem entweder abstrakten oder besser noch in einer konkreten Persönlichkeit verkörperten *Ideal*. Weitaus am stärksten beim J_3-Typus ist dasjenige wirksam, was Grunsky die "Blutwelt" nennt. Die tiefliegenden vorrationalen Instinkte, Gefühle und Triebe, von denen der J_3-Typus sich leiten lässt, sind eben diejenigen der *Gemeinschaft*, der er angehört. Sie stammen nicht aus Ideen, Erziehungseinflüssen oder Veranstaltungen des Verstandes, sondern aus angeborenen Triebkräften, bildlich gesprochen aus der "Blutwelt." Diese Zusammenschlüsse des J_3-Typus, die in der "Blutwelt," also in einer gleichartigen angeborenen Anlage gründen, sind von allen die festesten und unerschütterlichsten.

Ich habe hier die Beziehungen der Typenforschung zu den Nachbargebieten zunächst an der Hand der von mir vertretenen Integrationstypologie dargestellt, weil mir diese am nächsten liegt, und weil sie von den typologischen Systemen innerhalb der Fachpsychologie das *älteste* und am längsten durchgearbeitete ist. Es wird aber mit jedem Jahre deutlicher, dass die Verschiedenheit unserer Typologien immer zunehmend einer *Einigung* Platz macht. So ist deutlich erkennbar, wie die Göttinger Schule von N. Ach und seinen Mitarbeitern (Firgau u.a.), die sich zuerst der Typenlehre von E. Kretschmer, später derjenigen von C. G. Jung angeschlossen hat, nun auf eine Einigung mit der Integrationstypologie hindrängt. Dass das *Jung*'sche Schema "extravertiert-introvertiert" viel zu einfach ist, um die menschlichen Grundformen in sich aufnehmen zu können, diese Erkenntnis hat sich auch der Göttinger Schule bei der Fortsetzung ihrer Arbeit aufgedrängt. Ausser dem extravertierten und introvertierten unterscheidet Ach neuerdings noch einen *hyperextravertierten* Typus. Wäre dies nur ein Extravertierter besonders hohen Grades, dann brauchte man ihn vom vom "Extravertierten" nicht zu unterscheiden. Die extravertierte

Gruppe enthält ja alle möglichen Grade der Extraversion, darunter natürlich auch die *hohen* Grade. Tatsächlich ergibt sich die Notwendigkeit, auch noch *diesen* Typus besonders herauszuheben, daraus, dass er eben nicht einfach ein extravertierter ist, sondern *etwas qualitativ anderes*. Der "Hyperextravertierte" stimmt überein mit dem S_1-Typus der Integrationstypologie. So bahnt sich hier eine Einigung an. Wäre der "Hyperextravertierte" nur ein *besonders stark* Extravertierter, so müsste man ja eigentlich auch eine besondere Gruppe des "Hyperintrovertierten" erwarten. Aber diese Gruppe aufzustellen, hat noch niemand Veranlassung gehabt, weil hier eben die *qualitative* Abweichung fehlt, die die Hervorhebung der sog. "Hyperextravertierten" notwendig machte. In ähnlicher Weise bahnt sich auch eine Einigung an zwischen der Integrationstypologie und der sog. Erbcharakterologie Pfahlers. Von G. Pfahler wird jetzt besonders der Unterschied der "festen und fliessenden Gehalte" hervorgehoben. In diesem Sinne unterscheiden sich aber gerade die Typen J_3 und J_1 (bzw. S_1) der Integrationstypologie, und auch schon in deren Vorstufe, in der Eidetik (1925), war von mir auf diesen Unterschied hingewiesen worden.

Die neueste Weiterbildung der Typenlehre Kretschmers lässt nun auch ihre Beziehung zur Integrationstypologie deutlich hervortreten. Die frühere *Zwei*teilung wurde von Kretschmer jetzt durch eine *Drei*teilung ersetzt: der pyknische Typus wird in Verbindung gebracht mit der Anlage zum zirkulären Irresein, der leptosome mit der zur Schizophrenie, der athletische mit der zur Akromegalie und Epilepsie. Hieraus geht klar hervor, dass es sich bei diesen Formen mehr um *Typen der Krankheitsveranlagung* als eigentlich um *Normaltypen* handelt. Der leptosome Typus im Sinne von Kretschmer stimmt überein mit dem Stillerschen Habitus, der schon von jeher als besonders disponiert für Tuberkulose galt. Auf *dieselben* krankheitsätiologischer Zusammenhänge stösst nun aber von anderer Seite auch die Integrationstypologie. Denn nach ihren Befunden zeigt der lytische S-Typus eine Korrelation (1) zu der *Tuberkulose*, (2) zu verschiedenen psychopathologischen Zustandsbildern—ausgenommen das zirkuläre Irresein—,darunter vor allem zur *Schizophrenie;* eine verbreitete Untergruppe des S-Typus (S schizoform) zeigt Erscheinungen, die an Schizophrenie anklingen. Die Kretschmersche Typenlehre teilt die Menschen tatsächlich ein nach ihrer *Veranlagung zu bestimmten Formen der Erkrankung*. Hat man sich dies klar gemacht, dann fallen die zahlreichen Schwierigkeiten hinweg, welche auftreten, wenn man die Kretschmersche Lehre als eine Typologie ansieht, die die *ganz normalen* Typen mitumfasst. Als-

dann ist nämlich die sog. schizothyme Gruppe eine *völlig uneinheitliche* Klasse, die die heterogensten Typen—ja die grössten typologischen Gegensätze, die es überhaupt gibt—*zugleich* umfasst: J_3 und S_1, d.h. den *strukturfestesten* und den *strukturlockersten* Typus. Weil die Kretschmersche Typologie keine Normaltypologie, sondern eine *Typologie konstitutioneller Krankheitsursachen* ist, darum liefert sie auch der vergleichenden Völkerpsychologie, der Kulturpsychologie und Rassenkunde keine Stützpunkte; denn die (ätiologischen) Typen Kretschmers kommen, wie von ihm selbst mit Recht betont wird—gleich den zugehörigen Erkrankungen—*überall* vor. Die Integrationstypologie dagegen enthält für die genannten Disziplinen die typologischen Grundlagen. Eine grosse und ausserordentliche Bedeutung besitzt die Kretschmersche Lehre für die *Krankheitsätiologie* und damit für die Medizin; für die Normalpsychologie nur indirekt und nur insofern, als auch in der Normalpsychologie der breite Grenzbereich nach dem Pathologischen hin immer wichtiger geworden ist und auch in ihr berücksichtigt werden muss. Es ist somit ein bleibendes Verdienst Kretschmers, und gerade das hat wohl seinem Werk mit Recht eine so starke Beachtung gesichert, dass er die hohen Trennungsmauern zwischen den Fakultäten von der *Medizin* her zu durchbrechen unternahm, was die deutsche Fachpsychologie von der *Philosophischen Fakultät* aus versucht. Aber hierbei sehen die Dinge eben etwas anders aus, als sie sich unserem revolutionären Bundesgenossen bei dem Durchbruch von der *Medizin* her darstellen. Insbesondere ergibt sich *für uns* die Notwendigkeit, darauf zu halten, dass nicht *kerngesunde normale* Formen in eine ihnen wesensfremde Beziehung zu *Krankheitsbildern* gebracht und dadurch verdunkelt oder verzeichnet werden; ein Gesichtspunkt, der übrigens auch von manchen Psychiatern, z.B. von O. Bumke, gegenüber der Typenlehre Kretschmers, soweit sie als *Normaltypologie* auftritt, immer geltend gemacht worden ist.

Weil unsere deutsche Bewegung die bevölkerungspolitschen Probleme an der *Wurzel* angreift, darum betont sie auch aufs stärkste die *Erbhygiene*. Diese Tatsache spiegelt sich auch in der heutigen deutschen Psychologie. Pfahler betrachtet *alle* menschlichen Grundtypen—insbesondere auch die festen und fliessenden—als *erbmässig* bedingt, und er bezeichnet darum jetzt die Lehre von den menschlichen Grundformen schlechthin als "*Erbcharakterologie.*" Ich selbst bin in nunmehr 20 jähriger typologischer Arbeit zu einer etwas abweichenden Ansicht geführt worden: Die Strukturfestigkeit ist, wie anscheinend überhaupt alle Vorzüge im biologischen Sinn, *erblich* verankert. Auflockerungen

dagegen und Auflösungen dieser Strukturen, und darum auch die aufgelockerten und aufgelösten Strukturtypen, können als Wirkung der allerverschiedensten äusseren Schädigungen und *Umwelteinflüsse* auftreten. Diese haben auf die menschliche Grundstruktur so gut wie nie, im biologischen Sinne verstanden, einen *verbessernden*, sehr oft dagegen einen *verschlechternden* Einfluss. Feste Strukturen sind *erbdeterminiert*, lockere und aufgelöste Strukturen können erbdeterminiert, aber auch *umweltdeterminiert* sein. Tatsächlich macht auch unsere deutsche Bevölkerungspolitik keine Voraussetzung, die über das soeben Erwähnte hinausginge. *Weitergehende* Anschauungen, die die Erbbedingtheit *aller* wesentlichen Eigenschaften schlechthin behaupten, dürften sich als *Hyperbeln* erweisen, und, wie alle *Übertreibungen, auf die Dauer* für die Bewegung eher eine Hemmung als eine Förderung bedeuten; in diesem Falle vor allem deshalb, weil jedes sachlich unbegründete "Zuviel" in der Propaganda für den Erblichkeitsgesichtspunkt die so notwendigen ärztlichen und kulturpolitischen Massnahmen diskreditieren und lahmlegen kann, die auf die Heilung der von der *Umwelt* hervorgerufenen Schäden gerichtet sind.

Durch die Berücksichtigung dieser *verschiedenen* Abhängigkeit der Typen von Erb- und Umwelteinflüssen konnte auch ein alter Streit innerhalb der Sinnespsychologie geschlichtet werden, derjenige zwischen dem *Empirismus* von Helmholtz und dem *Nativismus* von Hering. Zunächst hat die typologische und alterstypologische Behandlung der Wahrnehmungslehre in den Helmholtzschen Anschauungen eine wesentliche Lücke ausgefüllt. Helmholtz' und von Kries' Ansicht, dass *Vorstellungen* zu einem Baustein der Wahrnehmungswelt werden können, ist entgegenzuhalten, dass doch nicht Vorstellungen *beliebiger Art* hierzu befähigt sind. In der *Eidetik* wurde von uns gezeigt, dass in der frühen Jugend, in der ja nach Helmholtz der Aufbau der Wahrnehmungswelt erfolgt, in weiter Verbreitung Vorstellungen vorkommen, die den Charakter von *Empfindungen* besitzen, d.h. eben eidetisch sind und aus diesem Grunde zu einem Bestandteil, einem Baustein, unserer Wahrnehmungswelt werden können. Tatsächlich nun weichen die Wahrnehmungsvorgänge stark eidetischer Jugendlicher wesentlich ab von denen der Erwachsenen und zeigen in ihrem Verhalten eine weitgehende Ähnlichkeit mit den eidetischen Phänomenen. Aber diese Eigentümlichkeit der Wahrnehmungsvorgänge in der Jugend war von uns an den *stärker eidetischen* unter den Jugendlichen festgestellt worden. Diese gehören aber von früher Jugend an und auch später dem J_1-oder S-Typus an. Für diese Typen mit weicher, lockerer und eindrucksfähiger

Struktur trifft also die Anschauung von Helmholtz zu, dass die Wahrnehmungsvorgänge nicht von Anfang an fertig sind, sondern im individuellen Leben eine Entwicklung durchmachen, und dass eidetische Phänomene als Bestandteil in sie eintreten. Spätere Nachuntersuchungen an Jugendlichen, die von Anfang an stärker in der Richtung des J_3-Typus veranlagt und darum auch *uneidetisch* sind, haben von diesem besonderen Charakter der frühjugendlichen Wahrnehmungsvorgänge und ihrer allmählichen Entwicklung *schlechterdings gar nichts* erkennen lassen. Hier sind die Wahrnehmungsvorgänge, wie die nativistische Theorie Herings lehrt, von Anfang an fertig, d.h. allein durch angeborene Anlagen bestimmt. Wir erkennen jetzt, weshalb der Streit zwischen Helmholtz und Hering nie entschieden worden ist: *Beide* haben recht, nur eben für *verschiedene* Menschentypen. Die empiristische Lehre trifft zu für die Individuen mit weicher, eindrucksfähiger (J_1 oder S_1)-Struktur, die nativistische dagegen für die Angehörigen der härteren Strukturen (J_3, J_3/J_2, unter Umständen auch noch J_2). Der Helmholtzsche Empirismus betont die *Umweltdeterminanten*, der Heringsche Nativismus die *Erbdeterminanten*.

Wie in diesem Bereiche der Wahrnehmungsvorgänge, so erweisen sich *auch sonst* die weichen, eindrucksfähigen J_1- und S_1-Typen als überwiegend umweltdeterminiert, die härteren J_3- und J_3/J_2-Typen als überwiegend erbdeterminiert. Hieraus folgt, dass das Verhältnis "Erbdeterminiertheit: Umweltdeterminiertheit" nicht für *alle* Grundformen gleich, sondern sehr verschieden ist, und dass darum auch die Frage der Erbdeterminiertheit für die verschiedenen menschlichen Grundformen *gesondert* behandelt werden muss.

An diesem Beispiel dürfte schon klar geworden sein, dass eine echte *Normaltypologie* sich auch in der *Jugend- und Entwicklungspsychologie* bewährt und hier wesentliche Aufklärungen zu geben vermag. In der Tat hat die Eidetik und die Aufdeckung der eidetischen Phase des kindlichen Seelenlebens die Aufmerksamkeit auf die Besonderheit dieser frühkindlichen Entwicklungsstufe hingelenkt. Die ganz *normalen* Formen der eidetischen Jugendanlage entspringen der J_1-Struktur. Entsprechend hat sich überhaupt gezeigt, dass die einzelnen Phasen der Jugendentwicklung durch die oben angegebenen menschlichen Grundformen charakterisiert werden können. Das ist nicht dahin zu verstehen, dass die Grundform wechselt. Sie ist in allen Phasen erkennbar, wird aber in jeder *nüanciert* im Sinne *einer* der Grundformen; diese Nüance setzt sich dann auf die lebenslänglich bleibende Grundform auf, wie ein Oberton auf den Grundton.

Diese Nüancierung erfolgt in der *Kindheit* im Sinne der J_1-Struktur, im *Jünglingsalter* im Sinne von J_2 und beim *Erwachsenen* in der Richtung auf J_3. Zwischen diesen Hauptphasen sind kurze Auflockerungsphasen (S-Phasen) eingeschaltet, in denen die Umstellung erfolgt:

$$S-J_1-S-J_2-S-J_3.$$

Jede Phasenstruktur ist um so ausgeprägter (bzw. unausgeprägter), je näher (bzw. ferner) sie der lebenslänglich bleibenden Grundstruktur steht. Die Phase J_1 ist also bei den Menschen vom Grundtypus J_1 sehr ausgeprägt, bei solchen vom Grundtypus J_3 dagegen unausgeprägt. Nun konnte gezeigt werden, dass J_1 eine wesentliche Affinität besitzt zum *künstlerischen* Welterleben. Im Einklang hiermit findet man bei Individuen vom J_1- (oder auch S_1-) Typus in der Kindheit in weiter Verbreitung Eigentümlichkeiten, die in der Richtung künstlerischer Anlage liegen. O. Kroh mit seinen Schülern widerlegte die verbreitete Anschauung von der ästhetischen Unempfänglichkeit des Schulkindes. *Produktive* künstlerische Kräfte—innerhalb einer elementaren Ebene—wurden aus der Praxis von Erziehung und Unterricht heraus nachgewiesen von Erwin Heckmann und Leo Weismantel. Mit der Analyse dieser produktiven Kräfte beschäftigten sich Arbeiten aus dem Kreise des Verfassers, und neuerdings solche aus dem Jenaer Institut von F. Sander.

Aber wie bei dem Hering-Helmholtzschen Problem und bei der Charakterisierung der kindlichen Wahrnehmungswelt, so muss auch hier der Grundtypus berücksichtigt werden. Wie wirkt sich nun die kindliche Phasenstruktur J_1 mit ihrer Aufgeschlossenheit gegenüber den Inhalten der Aussenwelt, ihrer lebendigen Phantasie und daher rührenden Neigung zu produktivem Spiel *dort* aus, wo sich diese J_1-Phase auf die Grundstruktur J_3 aufsetzt? Hierüber und für die pädagogische Ausnutzung dieser Jugendphase geben wertvolle Hinweise die pädagogisch-psychologischen Versuche B. Petermanns im Bereiche einer "gewachsenen Raumlehre" mit "konstruktiver Durchdringung," d.h. mit Einführung eines *produktiven Konstruktionsspieles* im Geometrieunterricht.

Alle diese Bestrebungen, die Eigentümlichkeiten des Jugendgeistes auszuwerten, dienen dem Gedanken einer "organischen Erziehung," der in Fortbildung der Ansätze bei Pestalozzi von Jaensch, Kroh, Petermann, Sander, Tumlirz, u.a. vertreten wird; für die *frühe* Kindheit, in Fortbildung der Ansätze Froebels, namentlich von H. Volkelt.

Die Beziehung der Altersphasen zu den menschlichen Grundtypen

ermöglicht jetzt auch eine *allgemeine* Darstellung der Jugendpsychologie, trotz der Verschiedenheit der Typen. Man kann nun durch eine Art von *Koordinatentransformation* die *allgemeine* Darstellung auf jeden *besonderen* Typus transformieren, indem man sich gegenwärtig hält, dass z.B. beim J_1-Typus die J_1-Phase besonders ausgeprägt ist, usw. Das ist vor allem wichtig im Hinblick auf die *Entwicklungskrisen* (S-Phasen). Hält man sich an die verbreitetsten Darstellungen der Entwicklungspsychologie, so muss man annehmen, dass diese Krisen überall sehr stark und sehr deutlich hervortreten. Das liegt aber nur daran, dass heute der S-Typus stark vorwaltet, und dass dieser die S-Phasen der Entwicklung in starker Ausprägung darbietet. Dazu kommt, dass diese S-Phasen die Autoren als besonders "interessant" zur Darstellung gereizt haben. Aber gerade bei den durchschnittlich *gesündesten* und stabilsten Formen J_3 und J_2, die wir heute anstreben, treten diese Phasen stark in den Hintergrund.

Es erscheint auf den ersten Blick fast unmöglich, eine einheitliche, für unser ganzes Land gültige Darstellung der Jugendpsychologie zu geben, da sich die Entwicklungsvorgänge in unseren verschiedenen Landesteilen und Stämmen nicht gleichartig abspielen. Die Beachtung der Affinität zwischen den Entwicklungsphasen und den einzelnen Gundformen überwindet *auch diese* Schwierigkeit und gestattet, die *allgemeine* Darstellung durch eine entsprechende "Koordinatentransformation," ähnlich wie im vorigen Falle, auf die einzelnen *Stämme* und Bevölkerungsteile zu transformieren.

In jedem derselben walten bestimmte Grundformen vor. Da die Phasenstrukturen um so ausgeprägter sind, je mehr sie mit der bleibenden Grundform übereinstimmen, so muss z.B. in einem Bevölkerungs. teil, in dem der J_1-Typus vorherrscht, die kindliche J_1-Phase besonders ausgeprägt sein, und Entsprechendes gilt für die anderen Phasen.

Aus dieser kurzen Übersicht dürfte schon hervorgehen, dass die deutsche Psychologie eine enge Beziehung gewonnen hat zu den *allgemeinen Fragen* des Daseins, und dass sie namentlich auch wichtige Vorarbeit liefert zu ihrer praktischen Lösung. Damit knüpft sich auch wieder, mit Hilfe der Erfahrungserkenntnis, das Band zwischen Psychologie und Philosophie, das eine ausschliesslich spezialwissenschaftlich verfahrende Psychologie und eine wirklichkeitsferne Philosophie des 19. Jahrhunderts zerschnitten hatten, und es bahnt sich jetzt eine zunehmende *Einigung* an zwischen den bis vor kurzem noch so feindlichen Schwestern.

In dieser Krise erwarb sich ein unvergängliches Verdienst Felix

Krueger und die von ihm geführte Leipziger Schule. Die Zeit liegt noch nicht weit zurück, in der bei uns die öffenliche Meinung und die Presse geradezu die Wiederausschaltung der Psychologie forderten. Zu den "Zersetzungserscheinungen" des 19. Jahrhunderts, die wir heute bekämpfen, so wurde damals gesagt, gehört namentlich auch die "Atomisierung" der Seele durch die Psychophysik und die nach dem Vorbild der Naturwissenschaft verfahrende Psychologie. Zur Widerlegung dieser unberechtigten Vorurteile hat besonders viel Kruegers Ganzheitspsychologie und Gefühlslehre beigetragen. Hier wurde mit aller Deutlichkeit gezeigt, dass die von der psychologischen Analyse an der seelischen Wirklichkeit aufgedeckten Bestandteile nicht isolierte Bausteine oder Konstruktionsteile sind, die irgendwann einmal von der Natur nach Art von Maschinenteilen zusammengefügt wurden, sondern nur *verschiedene Seiten einer ursprünglichen Einheit und Ganzheit.*

Gegenüber dem "abgelösten" Geist (S_2-Geist) des verklingenden Kultursystems wurde schon vor Jahrzehnten *Leben und Seele* in der Philosophie von L. Klages betont. Ihr bleibender wissenschaftlicher Ertrag war die Erforschung der seelischen Ausdruckserscheinungen, die heute von Strehle u.a. fortgesetzt, mit besonderem Erfolg aber von Ph. Lersch in Gang gebracht wurde, dem das grosse Verdienst zukommt, die Forschungsmethode in diesem Gebiet erstmals auf eine exakte experimentelle Grundlage gestellt zu haben.

Ein Hauptgrund für die Gegnerschaft zwischen Philosophie und Psychologie in der vergangenen Epoche war der, dass die positivistische oder mechanistische Psychologie keinen Raum mehr liess für den *Bereich der Werte*, der doch für die Philosophie ein Anliegen ist, auf das sie nicht verzichten darf. Wie eng die heutige deutsche Psychologie mit *Wert- und Kulturfragen* verknüpft ist, dürfte schon aus dieser kurzen Übersicht hervorgehen. Darum bahnt sich zwischen der Wertphilosophie und der Psychologie, deren Streit einst im Mittelpunkt der deutschen Geisteskämpfe gestanden hatte, heute eine Einigung an, die von der gegenwärtigen Psychologengeneration und von den Fortbildnern der Wertphilosophie H. Rickerts (A. Faust u.a.) wie überhaupt des Idealismus (A. Gehlen u.a.) in gleicher Weise angestrebt wird.

Es dient zugleich dem Problemgebiet der Wertphilosophie, wenn die Typologie vom Verfasser, von W. Hellpach u.a. für die vergleichende *Völker- und Kulturpsychologie* ausgewertet wird. Da jede völkische Kultur Ausdruck der in dem betreffenden Volke vorwaltenden *Grundform* ist, oder auch—wie in Deutschland, England und Frankreich—des fruchtbaren Spannungsverhältnisses *zweier* Grundformen, so gibt

die Typenlehre einen Schlüssel an die Hand zum Verständnis der Erscheinungen des Völkerlebens und zugleich eine Anweisung zur Verwirklichung von Werten. Das Fernziel dieser Bestrebungen ist eine reiche "konkrete" Menschheitskultur, in der alle Völker ihre Eigenart zu scharfer Ausprägung bringen und einander, gerade *dank* dieser Eigenart, achten und ergänzen—im Unterschied zu der ärmlichen "abstrakten" Menschheitskultur, die ausschliesslich auf das in *allen* Menschen Gleiche, d.h. vor allem das *Subalterne* in ihnen, begründet ist. So liegen z.B. die in Deutschland vorherrschenden Grundformen durchweg um den J_T-Typus herum, der zugleich der Phasentypus des *Jüngslingsalters* ist. Hieraus ergibt sich die Notwendigkeit, der deutschen Kultur bei ihrer artgemässen Neugestaltung einen starken Einschlag des *Jugendgeistes* zu erteilen und damit der vielfach eingetretenen *Vergreisung* unserer Lebens- und Kulturformen entgegenzuwirken.

Einen besonders wichtigen Fragenkems der Wertphilosophie bildete von jeher das *religiöse Problem* und die Vorbereitung der inneren religiösen Erneuerung (R. Otto u.a.). Dass die entschieden *biologische* Ausrichtung unserer heutigen Psychologie dem *religiösen* Anschauungskreis nicht zuwiderläuft, wie manche annehmen, sondern im Gegenteil der inneren religiösen Erneuerung den Weg bereitet, geht schon daraus hervor, dass die Psychologie die Erscheinungen der Auflösung und Zersetzung, sowie ihre Ursachen, ans Licht zieht und bekämpft, und dass sie den *Antipoden* des Auflösungstypus den Weg bahnt, den menschlichen Grundformen mit *innerer Tiefe*, deren Vorhandensein die unerlässliche anthropologische Vorbedingung und Vorbereitung einer innerlichen religiösen Erneuerung ist.

Kants Lehre vom *Apriori*, das unsere Erfahrungswelt bedingt, erhält die schon von J. F. Fries geforderte Ausgestaltung in *empirischer* Richtung durch die Erforschung der verschiedenen Denkformen. Nach der vom Verfasser vertretenen empirisch-psychologisch fundierten *Kategorienlehre* erfassen wir die verschiedenen Strukturen des Realen mittels der *gleichartigen* Strukturen unseres Auffassens und Denkens. Hiernach muss die Möglichkeit offen gelassen werden, dass es neben dem klar erkennbaren Bereich des Wirklichen Gebiete der Wirklichkeit gibt, die unserem Erkennen unzugänglich sind, weil unser Bewusstsein keine ihnen gleichgeartete Strukturen besitzt. Das Vorhandensein solcher vom Scheinwerfer des Erkennens nicht getroffenen "Dunkelräume" der Wirklichkeit verrät sich besonders deutlich in der Existenz der sog. *parapsychologischen Erscheinungen*, die nur als tatsächlich festgestellt werden können, während ihr eigentliches Wesen mittels der bisher zur

Anwendung gebrachten Kategorien nicht aufgeklärt werden kann. Um die unbezweifelbare Festellung solcher Erscheinungen mit exakten experimentellen Mitteln hat sich, ausser F. von Neureiter, namentlich das von E. Rothacker geleitete Bonner Institut, und daselbst besonders H. Bender, verdient gemacht.

Bei diesen Erweiterungen des Rahmens der Psychologie sind die *älteren Richtungen* nicht zu kurz gekommen, sondern haben im Gegenteil gesteigerte Bedeutung gewonnen. Das gilt insbesondere von der *Psychophysik*, die von G. Th. Fechner begründet, von H. Ebbinghaus, G. E. Müller und F. Schumann fortgepflanzt und von einem grossen Teil der deutschen Psychologen heute weiter gepflegt wird (von Allesch, Bender, Jaensch, Keller, Klemm, Kroh, Metzger, Pauli, Schole, Wirth, u.a.). Zugleich mit der gesamten Psychologie erfuhr auch sie eine stärkere *biologische* Ausrichtung. Psychophysische Tatbestände sind nach dem Ergebnis der typologischen Forschung Indikatoren der Persönlichkeitsstruktur; ja die ureigenen Probleme der Psychopsysik erfahren oft ihre tiefere Auflösung erst dann, wenn sie in in diesen weiteren Zusammenhang der Persönlichkeitsforschung hineingestellt werden, wie Verfasser für den *Lichtsinn* zeigte, durch die Erweiterung der von Kries' schen Duplizitätslehre und durch den Synthesenversuch zwischen der Hering'schen und Helmholtz'schen Farbenlehre.

Hiermit tritt die Psychophysik zugleich in eine enge Beziehung zur *Medizin*, in der für die Psychologie kaum ein anderes Nachbargebiet so wichtig zu werden verspricht wie die *prophylaktische Medizin und korrigierende Konstitutionstherapie*, zu der Walther Jaensch wesentliche Grundlagen gelegt hat. Gerade die tiefgehenden und besonders verhängnisvollen unter den *psychischen* Schäden sind eben grossenteils *psychophysische* Schäden, die als stark konstitutionell gewöhnlich bis in die Kindheit zurückreichen. Bei ihren Bestrebungen zur seelischen Aufartung des Volkes ist darum für die Psychologie die prophylaktische Medizin und Konstitutionstherapie die wohl wichtigste Bundesgenossin.

Die *praktische Psychologie*, Berufskunde und Berufsauslese nahm im Zusammenhang mit der allgemeinen Psychologie einen bedeutenden Aufschwung. Das grösste Verdienst hierbei gebührt ausser der privaten Initiative Einzelner, wie W. Moede, W. Poppelreuter, W. Schulz, Th. Valentiner u.a., in erster Linie dem grandiosen Werk der *deutschen Heerespsychologie*. Das Geheimnis ihres Erfolges liegt in der straffen, durch die militärische Leitung gewährtleisteten Zentralisation in

organisatorischer Beziehung, in Verbindung mit einer *vorbildlichen geistig-wissenschaftlichen Dezentralisation*, die es ermöglicht, dass dauernd Anregungen ebenso wie Angehörige aus sämtlichen wertvollen Arbeitsrichtungen des Faches in den Kreis der Heerespsychologie einströmen können und hier in weitherziger Weise aufgenommen werden. Trotzdem ist die *Einheitlichkeit der Ausrichtung* auf zwangloseste Art gewährleistet, und zwar, wie es in geistig-wissenschaftlichen Dingen auch nicht anders sein kann, durch die *in rein sachlicher Arbeit* gründende *einheitliche Ausrichtung der psychologischen Wissenschaft*.

Durch die Berücksichtigung typologischer und rassischer Gesichtspunkte in der praktischen Psychologie verlieren die von Moede, Valentiner, Herwig, Hische u.a. begründeten Methoden zur Prüfung einzelner Berufsleistungen nicht etwa ihre Bedeutung, wohl aber erfahren sie dadurch eine wertvolle Ergänzung. Mit Nachdruck muss vor allem dem Missverständnis entgegengetreten werden, die Berücksichtigung typologischer und rassentypologischer Gesichtspunkte könnte dahin führen, dass geeignete Berufsanwärter deshalb abgelehnt würden, weil sie unseren typologischen und rassentypologischen Leitvorstellungen nicht entsprechen. Die Einbeziehung der Typologie in die Berufsauslese dient vielmehr gerade deren Vertiefung und Verfeinerung. Dazu tritt allerdings für uns in Deutschland zugleich immer noch ein bevölkerungspolitischer Gesichtspunkt: Wir wollen allen, die mit *Auslese* zu tun haben den Blick für die wertvollen und ebenso für die weniger wertvollen und wertwidrigen Formen in unserem Volke schärfen. Wir wollen sogar, da irgendwie und irgendwann einmal *einem jeden* eine Aufgabe der Auslese obliegt, die Erziehung zu diesem "Blick" zu einem Gegenstand der *Allgemeinbildung* machen, und wir halten dies für so wichtig, wie nur irgend ein Bildungsgut der Vergangenheit. Die in dieser Weise geschulten praktischen Psychologen werden dann die bevölkerungspolitisch wichtigen Auslesegesichtspunkte schon unbewusst und instinkthaft immer mitberücksichtigen, ohne die jeweils besonderen Fachanforderungen der Berufsauslese zu vergewaltigen. Es muss hier wie mit der echten "Bildung" sein, die sich nie in den Vordergrund drängt, nie *allein* eine Handlung und Entscheidung in unserer Berufsarbeit bestimmt, aber bei allen diesen Entscheidungen leitend und richtunggebend im Hintergrund steht. Die Psychologie, die sich zugleich in den Dienst *bevölkerungspolitischer Eugenik* stellt, verdient es aber, wie nur irgend etwas, zu einem Gegenstand der "allgemeinen Bidung" zu werden.

L'ÉVOLUTION DE LA SENSATION LUMINEUSE

By Henri Piéron, Université de Paris
Editor, *L'année psychologique*
(Founded in 1894)

Lorsqu'une plage lumineuse est projetée sur la rétine, il naît, après un temps de latence appréciable, une sensation dont l'intensité perçue croît avec le temps avant de s'établir à un régime d'équilibre à peu près stable; et lorsque la stimulation cesse, l'intensité décroît et la sensation s'efface au bout d'un certain temps, laissant après elle des modifications d'excitabilité et des sensations nouvelles, dont les oscillations évoquent un système dont l'équilibre rompu se rétablit après des ondulations d'amplitude progressivement amortie.

Une stimulation lumineuse brève suffit pour engendrer une réponse sensorielle plus ou moins tardive, présentant une évolution d'établissement et d'évanouissement, dont l'image correspond assez bien à celle d'une secousse musculaire déclenchée par la stimulation électrique d'un choc d'induction.

Particulièrement propre à l'étude expérimentale, l'évolution de la sensation lumineuse est mieux connue que celle des autres modalités de réponse sensorielle avec lesquelles elle présente des analogies et des différences. Il est bien certain que l'intermédiaire de la réaction photochimique réversible qui conditionne l'excitation lumineuse intervient, avec des caractéristiques propres dans l'évolution de la sensation, à côté des propriétés du système de transmission nerveuse de l'excitation jusqu'aux couches cellulaires centrales qui sont le siège de la réponse perceptive. Au point de vue du mécanisme de la sensation, des données précises sont donc particulièrement importantes relativement à l'évolution de l'intensité de la réponse, et aux lois qui la régissent.

J'ai procédé, à cet égard, à tout un ensemble de recherches, dont peuvent se dégager quelques données générales, et quelques tentatives d'interprétation, pour ce qui concerne la sensation lumineuse. J'envisagerai succinctement la phase d'établissement, et la phase d'évanouissement, avant d'indiquer les données générales qui me paraissent pouvoir se dégager des faits.

I. L'ÉTABLISSEMENT

On a depuis longtemps constaté que, pour des intensités objectives égales de stimulation lumineuse, les intensités perçues étaient d'autant moindres que la durée d'action du stimulus était plus brève, dans une certaine marge du moins, ce qui impliquerait un "développement" de l'impression lumineuse, pour employer l'expression de Plateau, il y a plus d'un siècle.[1]

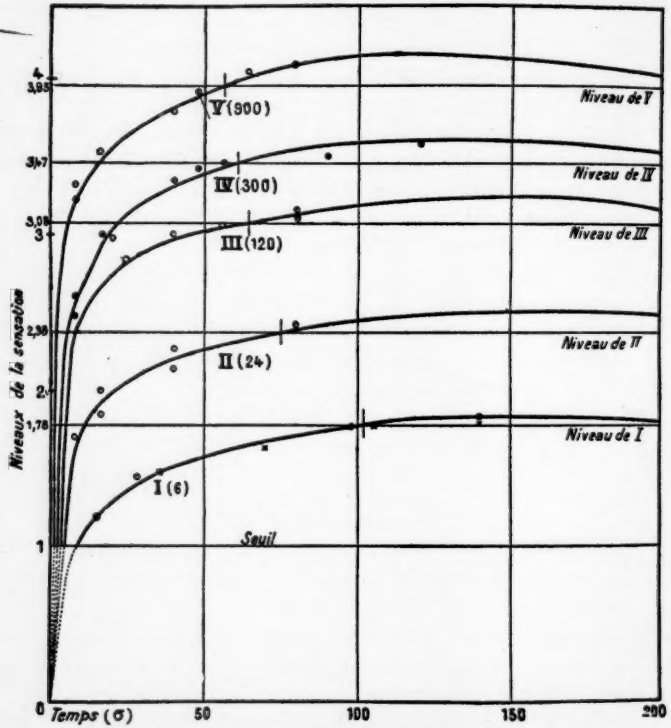

FIG. 1. COURBES D'ÉTABLISSEMENT DE LA SENSATION LUMINEUSE PAR EXCITATION FOVÉALE EN LUMIÈRE BLANCHE

Les niveaux de sensation sont inscrits en ordonnées logarithmiques. Les chiffres entre parenthèses indiquent l'intensité du stimulus en unités physiologiques (multiples de la valeur liminaire). D'après Kleitman et Piéron, *op. cit.*, 70.

[1] J. Plateau, *Nouveaux Mémoires de l'Académie royale des Sciences et belles Lettres de Bruxelles*, 8, 1834, 1 ff.

Le développement fut d'abord envisagé comme linéaire jusqu'à un plateau terminal, brusquement atteint (Swan) ou précédé d'un aplatissement progressif (Exner, Charpentier).

Broca et Sulzer[2] montrèrent qu'en réalité le plateau terminal ne se trouvait atteint qu'après une ondulation de prééquilibre analogue à celle que décrit l'aiguille d'un galvanomètre à la fermeture d'un courant, quand il possède une constante convenable d'amortissement. Et les recherches de Marion Hills et de Haas confirmèrent le fait.

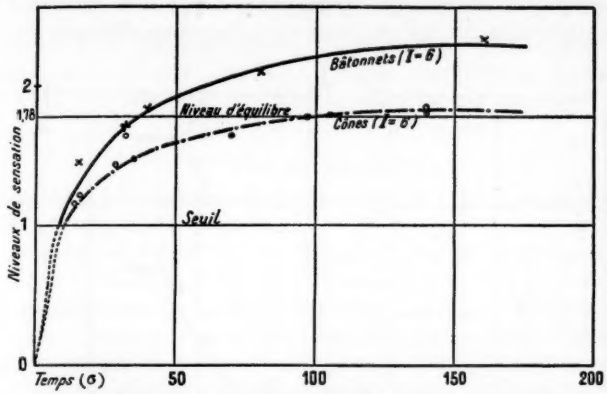

FIG. 2. COURBES D'ÉTABLISSEMENT DE LA SENSATION LUMINEUSE
Par excitation blanche fovéale (cônes) et bleue périphérique (bâtonnets) au même niveau physiologique d'intensité (6 fois la valeur liminaire).

C'est dans les déterminations que j'ai faites en collaboration avec N. Kleitman[3] qu'ont pu être fixées les lois d'établissement en fonction de l'intensité de stimulation, par détermination de l'intensité à donner à un stimulus agissant pendant 2 secondes, pour qu'il soit jugé égal au stimulus d'intensité fixe et de durée variable.

Les résultats généraux, représentés dans la figure ci-jointe (Fig. 1), permettent de relier la vitesse initiale d'établissement V (phase de croissance jusqu'au niveau d'équilibre, dépassé au cours de l'ondulation de prééquilibre) à l'intensité I, exprimée en multiples de la valeur liminaire, par une formule simple valable en première approximation (où a et k sont des constantes):

[2] A. Broca et D. Sulzer, La sensation lumineuse en fonction du temps, *J. de physiol.*, 4, 1902, 632-640.
[3] N. Kleitman et H. Piéron, Recherches sur l'établissement de la sensation lumineuse, *Année psychol.*, 25, 1924, 34-84.

$$V = a \log I + k.$$

L'excitation fovéale des cônes et l'excitation bleue des bâtonnets périphériques comportent des valeurs différentes des constantes (Fig. 2).

On a $V = 27.3 \log I + 13$ pour les bâtonnets
et $V = 4.6 \log I + 6$ pour les cônes

La vitesse d'établissement croît donc nettement plus vite avec l'intensité pour les bâtonnets que pour les cônes, avec dépassement beaucoup plus accentué du régime d'équilibre, et la différence doit tenir essentiellement aux processus photochimiques différents des deux

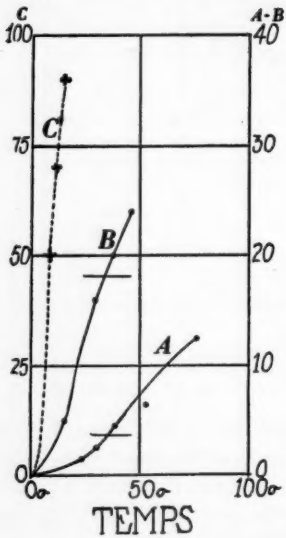

FIG. 3

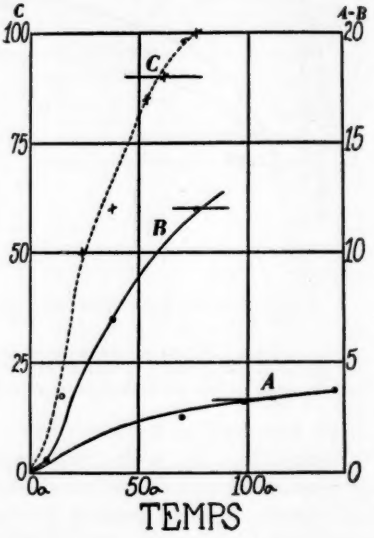

FIG. 4

FIG. 3. DÉBUT DE L'ÉTABLISSEMENT DE LA SENSATION LUMINEUSE PAR EXCITATION DES BÂTONNETS

(Sur une échelle arithmétique des intensités du stimulus durable jugé égal au stimulus de durée variable, cette durée étant inscrite en abscisse.)

À droite, échelle des intensités apparentes pour les courbes A et B, le stimulus valant 3, 5 et 18 unités psysiologiques. À gauche, échelle pour la courbe C, le stimulus valant 75 unités physiologiques (d'après des chiffres expérimentaux de Kleitman et Piéron).

FIG. 4. DÉBUT DE L'ÉTABLISSEMENT DE LA SENSATION LUMINEUSE PAR EXCITATION DES CÔNES FOVÉAUX

À droite, échelle des intensités apparentes pour les courbes A et B, le stimulus valant 6 et 24 unités physiologiques. À gauche, échelle pour la courbe C, le stimulus valant 120 unités physiologiques (d'après des chiffres expérimentaux de Kleitman et Piéron.)

systèmes récepteurs. La vitesse est exprimée dans ces formules par la réciproque du temps nécessaire pour atteindre la première fois le régime stable. Mais, ce régime correspondant à un niveau plus élevé quand augmente l'intensité de stimulation, la vitesse réelle de l'établissement augmente encore plus vite que ne l'indique la loi empirique précédente.

Si l'on acceptait la loi de Fechner, on aurait, en approximation grossière cette fois, pour la vitesse réelle Vet, une loi du type:

$$\text{Vet} = a\,(\log \text{I})^2 + k \log \text{I}.$$

La courbe d'établissement implique la forme en S, comme le montrent bien les figures ci-jointes (Figs. 3, 4), donnant trois séries de déterminations à des niveaux différents pour l'excitation des bâtonnets périphériques et trois pour l'excitation des cônes fovéaux.

II. L'Évanouissement

L'étude de l'évanouissement de la sensation est beaucoup plus difficile que celle de l'établissement.[4]

En procédant à des excitations de durée réglée, on peut déterminer chaque fois le niveau le plus haut qui sera atteint par la réponse sensorielle: un index fixe—le repère de la sensation voisine de durée fixe—est-il ou non atteint, c'est ce que l'on peut dire, et nous admettons que, pour le stimulus durant plus longtemps, les stades correspondent à ceux auxquels s'arrête la réponse pour des durées variables d'action. Mais il est impossible de suivre l'établissement lui-même, car, pour faire une comparaison, il faut un certain temps, et une observation de l'intensité perçue d'une lumière au bout d'un ou de deux centièmes de seconde ne peut être obtenue d'aucune manière.

Pour l'évanouissement, il n'est pas davantage possible de procéder à des observations directes. Pouvons nous utiliser une méthode analogue à celle qui a permis l'étude de l'établissement? Il faudrait régler l'interruption du stimulus et chercher, quand l'interruption dure un ou deux centièmes de seconde par exemple, le niveau correspondant de l'intensité perçue avec un stimulus de comparaison. Mais faire cesser une interruption, c'est faire agir une nouvelle stimulation, avec ses modalités propres d'établissement, donnant une réponse croissante qui vient recouvrir la réponse décroissante du stimulus précédent. On perçoit alors un papillotement, dans lequel la fixation des niveaux supérieur et inférieur de luminosité apparente au cours de l'oscillation

[4] Cf. H. Piéron, L'évanouissement de la sensation lumineuse, *Année psychol.*, 35, 1934, 1-49.

est à peu près complètement impossible. Il y a toutefois un point qui peut être fixé dans la décroissance de la sensation, c'est celui qui correspond à la valeur d'un échelon différentiel. En effet, tant que la décroissance reste inférieure à la valeur minima perceptible, les interruptions restent ignorées, et l'impression lumineuse est stable en dépit des intermittences. Si l'on détermine la valeur limitée de ces intermittences juste imperceptibles (fréquence critique de papillotement), on précise le taux de décroissance de la sensation au cours de l'intervalle de temps ainsi déterminé, taux correspondant à la valeur du seuil differentiel. Un second point peut, non sans difficulté, être précisé, c'est celui qui correspond à l'évanouissement total de la sensation (avant les oscillations ultérieures d'images consécutives).

J'ai donc, dans ce but, recherché la vitesse de rotation d'un disque, portant un secteur lumineux d'étendue définie, nécessaire pour donner à l'oeil immobile l'impression d'un anneau complet, le retour de la sensation se faisant au moment juste où cesse la sensation engendrée par la stimulation précédente (avec un décalage supposé à peu près constant des retards de latence). J'ai trouvé ainsi des valeurs de 150 à 180σ, et des persistances totales un peu plus grandes pour des éclairements plus intenses, mais avec une faible marge de variation.

D'autre part, une étude de l'influence de la surface d'excitation sur la valeur de la fréquence critique de papillotement m'a permis de préciser le début de la décroissance en utilisant la variation de l'échelon différentiel dont on connaît la relation avec la surface:

Pour les surfaces allant de 1 à 1000, les intermittences, au cours desquelles la décroissance atteignait juste la valeur du seuil différentiel, allaient de 31,5 à 13,8σ, la valeur relative de l'échelon allant de 0,100 à 0,0277. Un échelon représentant 10% du niveau d'équillibre de la sensation avant l'intermittence est franchi en 31,5σ; un échelon représentant 2,77% en 13,8σ.

Des valeurs intermédiaires (surfaces 10 et 100) donnent des échelons relatifs de 5,26 et 3,70%, franchis en 23,3 et 17,3σ.

Nous obtenons ainsi des repères qui nous permettent, en partant d'un niveau de sensation égal à 100, de pointer au bout de combien de temps d'interruption la décroissance a atteint le niveau 97,23, le niveau 96,3, le niveau 94,76 et le niveau 90.

L'évanouissement total exigeant au moins 150σ, on peut, avec les points initiaux et le point terminal, esquisser l'allure générale de la décroissance. Comme le montre la figure ci-jointe (Fig. 5), il s'agit encore d'une courbe en S.

La vitesse d'évanouissement augmente avec l'intensité de la stimulation préalable, d'après les déterminations faites sur la *persistance indifférenciable* (fréquence critique de papillotement), mais avec la cause d'erreur de la variation de l'échelon différentiel.

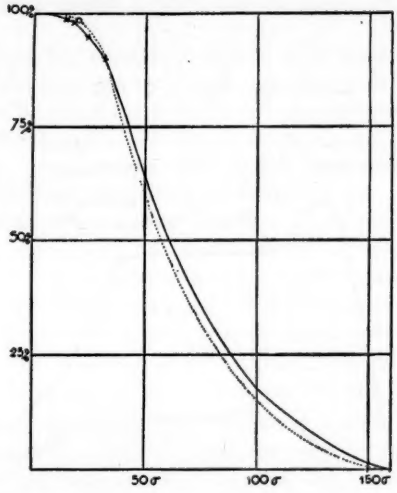

FIG. 5. COURBE D'ÉVANOUISSEMENT DE LA SENSATION LUMINEUSE

En abscisse, le temps; en ordonnée, le niveau de la sensation (en valeur relative pour cent.). Les points marqués sur la courbe en trait plein indiquent des valeurs obtenues pour le franchissement de l'échelon différentiel—variable avec la surface—en valeur relative (le niveau initial fait égal a 100) en négligent l'influence des variations de brillance efficace. La courbe en pointillé apporte une correction approximative faisant intervenir l'action de la brillance sur la variation avec la surface du seuil différentiel. (*Année psychol.*, 35, 1934, 39.)

Tandis que le niveau stable est atteint plus tôt quand le stimulus est plus intense, le retour au zéro est atteint un peu plus tard; toutefois, la différence étant petite, la décroissance est cependant accélérée, puisque, dans un temps presque égal, la descente s'effectue d'un niveau plus élevé, et le nombre des échelons de sensation qui sont ainsi descendus est plus grand.

Mais une certaine dissymétrie se manifeste, en ce que l'intensité du stimulus accélère davantage la croissance de la réponse que sa décroissance au cours de l'évanouissement spontané.

III. Schéma Évolutif et Hypothèses

Nous avons, dans la figure ci-jointe (Fig. 6), schématisé l'évolution d'une sensation lumineuse provoquée par deux stimuli d'égale durée et d'intensité différente, montrant comment varie la latence, l'établissement (avec son ondulation de prééquilibre) et l'évanouissement, ainsi que la décroissance initiale dans la marge d'un échelon différentiel.

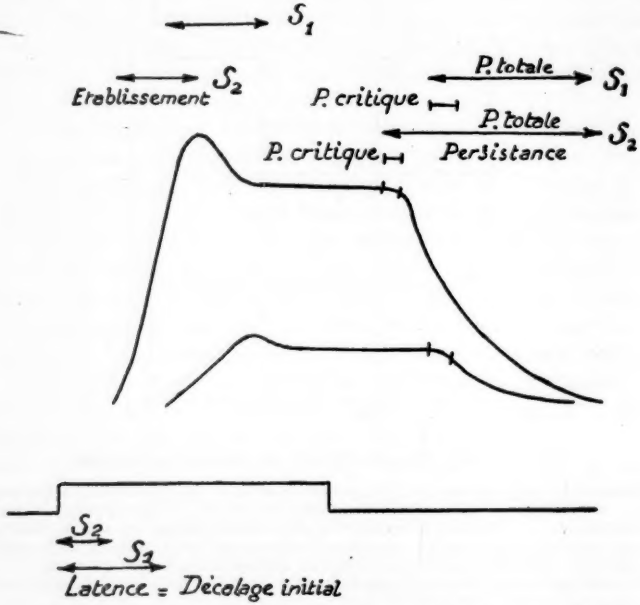

Fig. 6. Schéma de l'évolution de deux sensations lumineuses provoquées par une stimulation d'inégale brillance, dont la durée est indiquée sur la ligne inférieure
Les durées des latences sont indiquées par des flèches ainsi que les durées d'établissement, et celles de persistance (p. critique et p. totale) pour S^1, proche du seuil, et S^2 plus intense. (*Année psychol.*, 35, 1934, 45.)

Dans cette évolution, les facteurs dépendant de la réaction photochimique réversible qui conditionne l'excitation jouent certainement un rôle essentiel; et c'est à la dissymétrie des processus photochimiques dans la phase d'éclairement, et des processus antagonistes dans la phase de repos, que doit être due la dissymétrie d'action de l'intensité du

stimulus lumineux sur les vitesses respectives d'établissement et d'évanouissement.

Mais l'allure générale de l'évolution, très semblable à celle de la contraction musculaire, doit faire intervenir un processus analogue de recrutement, les "esthésio-neurones" corticaux correspondant, pour la réponse sensorielle, aux unités contractiles pour la réponse musculaire.

J'ai montré que la variation du niveau de la sensation en fonction de l'intensité du stimulus s'accordait très bien avec l'hypothèse de l'intervention d'un nombre croissant d'éléments cellulaires inégalement excitables dans la réponse des centres sensoriels de l'écorce, recrutement qui peut être fonction, soit de l'augmentation préalable du nombre des neurones périphériques intéressés par l'excitation (ce qui paraît être le cas pour la réponse auditive), soit de la réduction de l'intervalle entre influx consécutifs quand la fréquence des influx répétitifs s'élève avec l'intensité (la différence d'excitabilité des esthésio-neurones se ramenant alors à une différence dans la vitesse de dissipation des effets produits par un premier influx isolément inefficace, et auxquels doivent s'ajouter les effets produits par un second influx). C'est ce qui se passe dans le cas de la réponse visuelle.[5]

L'établissement de la sensation correspond alors à la phase du recrutement progressif d'esthésio-neurones gagnés par l'excitation, jusqu'à un plafond correspondant au total des cellules susceptibles d'entrer en jeu sous l'action du stimulus, à un niveau donné d'intensité, l'évanouissement à la phase du lâchage progressif de ces esthésio-neurones, dont certains tiennent plus longtemps.

Mais, est-ce dans les conditions du recrutement cortical que peut s'expliquer cette mise en jeu ou cette mise en repos progressive, il ne le semble pas.

Il doit y avoir là un effet, en ce qui concerne la réponse visuelle, de la variation de fréquence des influx au début de l'action du stimulus et après sa cessation.

La fréquence d'équilibre, pour un stimulus donné, n'est pas un effet instantanément réalisé, et il y a, dans le nerf, une phase d'établissement, d'augmentation de fréquence, comme il y a, avant l'extinction, une phase d'influx se raréfiant; les données de Hartline sur la réponse d'une fibre unique du nerf optique de la Limule le mettent nettement en

[5] Cf. H. Piéron, Le probléme du mécanisme physiologique impliqué par l'échelon différentiel de sensation, *Année psychol.*, 34, 1933, 216-236.

évidence. Aussi me parait-il que, si les conditions propres du recrutement des esthésio-neurones interviennent pour rendre compte de la courbe en S, (correspondant à l'intégrale de probabilité) de l'accroissement du niveau d'équilibre de la sensation lumineuse en fonction de l'intensité du stimulus—d'après la fréquence corrélative des influx,—en revanche l'évolution dans le temps, avec variation progressive initiale et régression terminale, si elle est toujours nécessairement due à l'évolution du recrutement des esthésio-neurones dont le nombre conditionne le niveau de la réponse sensorielle, a, comme cause profonde, la variation du "message sensoriel" transmis commandant le recrutement.

Car le retard de mise en jeu des esthésio-neurones peut être d'autant plus grand qu'il s'agit d'élements plus sensibles (pouvant intégrer deux influx séparés par un intervalle plus long, ayant en quelque sorte une mémoire plus tenace, une dissipation ralentie), et ce sont les éléments mis en jeu seulement par les stimuli les plus intenses, quand les deux influx devant ajouter leurs effets sont séparés par l'intervalle le plus petit, au moment où la fréquence du nerf atteint son plafond, qui ont obligatoirement la latence la plus brève de mise en jeu. Quand ceux-là sont intéressés par l'excitation, tous devraient l'être. Si tous ne le sont pas simultanément, c'est donc seulement parce que la fréquence des influx n'atteint pas d'emblée son plafond, ou tout au moins son régime d'équilibre.

STUMPF'S "INTRODUCTION TO PSYCHOLOGY"

By HERBERT S. LANGFELD, Princeton University
Editor, *The Psychological Review*
(Founded in 1894)

Stumpf, unlike his chief contemporary, Wundt, never wrote a systematic psychology, so that the only persons who have been able to obtain a unified and comprehensive knowledge of his views on the important questions of psychology, and a clear understanding of his methods of treatment of the various problems, are those who have heard his lectures in his introductory course. I shall therefore present in this paper a description of Stumpf's views as he outlined them in his course in psychology at the University of Berlin during the winter semester of 1906-1907.[1] It will be useful to remember that it was during these two years that Stumpf published three of his most important theoretical papers, namely, *Über Gefühlsempfindungen*, *Erscheinungen und psychische Funktionen*, and *Zur Einteilung der Wissenschaften*, since their fundamental principles had already become an integral part of his pattern of thought.

Psychology is engaged in the observation of daily life, in order to raise such observations to a science through methodical treatment. This is the first statement of the course and it reveals Stumpf's respect for an empirical psychology as against the deductive treatment of most of his predecessors.

After a short historical excursion, Stumpf asked what is psychology, what does it do, and what are its methods? The simplest definition of psychology, he stated, would be that it is the science of the soul. Albert Lange had proposed a psychology without a soul, but Stumpf would not go so far as to say that it is impossible to determine the nature of the soul. The definition he gave, however, is his well-known statement that psychology is a science of the elementary psychic functions, those functions which are directly given and which by analogy with ourselves we conclude exist in other beings. That there

[1] Professor Kurt Koffka and I attended these lectures in the same year. This article represents my impression of the course, aided by the extensive lecture notes of Professor Koffka.

are psychic *functions* even the materialists do not deny, and it is, therefore, not necessary to follow Brentano's lead and speak of psychology as a science of psychic *phenomena* in order to avoid dispute.

The sensory phenomena, as well as the psychic functions with which they are in the closest relation, are given to us directly. The natural sciences start from the phenomena as does psychology, but the former are not concerned with the phenomena as such. The natural sciences leave the phenomena and arrive at forces and objects, which are presumed to cause the phenomena, while the psychologist is concerned with the functions as the material of his science. Psychology, however, as traditionally taught, is also interested in the phenomena as the content of perception and this part of psychology is called phenomenology.

Continuing his definition, Stumpf stated that psychology differs from the other mental sciences, such as the social sciences, in that the latter are concerned with the special manifestations of psychic activity, while psychology deals with psychic functions in general; in other words, with those aspects which are found in every individual as well as in infra-human organisms in so far as we are able to draw conclusions from their behavior.

The great advantage of psychology over the other sciences lies in the fact that the objects of psychology are a matter of immediate knowledge, while the innermost characteristics of the occurrences of nature remain unknown. Psychology also has a practical significance in that it is fundamental to logic, ethics, esthetics, pedagogy, and social and political sciences. Stumpf added, however, that even today there are some lawyers who are skeptical of traditional psychology and have developed their own psychology. To an extent one must agree with the critics of applied psychology, for general psychology has not yet reached that state of perfection necessary for the best application of its principles in practical affairs.

Methods of psychology consist in abstract reflection, external and internal observation (*Beobachtung*), experiment, and statistical method. We call that perception (*Wahrnehmung*) external which has only sensory data as content, and internal which has functions as content, such as perception itself, comparison, judgment, conclusion, feeling, and willing. It is not necessary to have two kinds of consciousness. The same consciousness can at one time be directed toward sensory content and at another time toward function. In answer to the criticism that functions are not directly experienced, Stumpf replied that we use the term internal perception to designate an experience of a very definite and

distinct kind. Furthermore, it is possible for anyone who interests himself in the details of internal perception to convince himself of the reality of such functions.

Stumpf, sensitive to the criticisms against psychological methods, continued with the statement that science requires observation; but, he asked, is there such a thing as psychic observation? Can we direct our attention upon psychic functions? Stumpf believed that we can. When we observe our own activity, however, a new activity occurs which alters the first activity either intensively or qualitatively. Wundt desired to substitute experiment for observation. Experiment, however, is no substitute for observation but rather includes the latter—a fact which Wundt later acknowledged. The solution of the problem of observation comes from J. S. Mill, who stated that psychic activity must be examined by means of primary or immediate memory. Brentano also emphasized this form of observation, although he did not distinguish between primary and secondary memory. Stumpf considered that it is only the primary memory which is sufficiently trustworthy for observation. It will be seen from these remarks that Stumpf meant introspection by such observation, although he did not use the term in this place. Perception of function he defined as a mere awareness without analysis, a distinction which he likened to Locke's differentiation between sensation and reflection. Stumpf's introspection or internal observation must not be confused with systematic introspection of the laboratory type. For Stumpf, the psychologist in such observation may be both subject and experimenter. Stumpf went on to state that the data from this method of observation reveals only a small part of mental life. To this method must be added verbal description and the observation of movement and action.

In a comparative method it is necessary to obtain as simple a situation as possible. He believed, for this reason, that child and animal psychology are especially fruitful. Some of the outstanding studies in these fields he mentioned included the 'enormous' literature on child psychology which Stanley Hall inspired. Stumpf also sketched the work which had been done in the abnormal and supernormal and in the social fields. He warned against an exaggeration of the value of the investigations of pathological cases and expressed skepticism of a scientific study of alleged spiritism and telepathy.

Stumpf defined the folk-mind as the common characteristics of the individuals who make up the group, characteristics which have developed through hundreds of years of social intercourse, but which

should not be hypostatized. There is nothing over and above the individuals who live together except the peculiar psychological characteristics that this common life necessitates.

Stumpf concluded his description of objective or comparative psychology with the remark that this method was being much overworked. The indirect method, he stated, can never supplant that of introspection. In fact, progress in objective psychology without a corresponding subjective check is dangerous.

In regard to the establishment of laws, Stumpf stated that it is an especial triumph of science when it can predict a complicated event. Such predictions seldom happen and can not yet be expected in psychology (this statement has special interest in view of the present work of Hull). Stumpf said, however, that one should be able to demand that psychology, in judging the average success of a pedagogical measure, should predict the probability of such a success.

Since Herbart, many investigators have endeavored to make psychology as exact a science as physics. Much impetus to this movement was given by Wundt's physiological psychology. Stumpf listed the new aids to an exact science as anatomy, experiment, measurement, and mathematics. It must be held in mind, he added, that the fundamental method is internal observation and that even in experimental work introspection is not only invaluable but that many experiments have been useless because either the experimenter or the subject was not thoroughly trained in observation. There then followed a long description of experimental work.

Stumpf believed that extensity of psychic functions can not be measured. Duration, however, is possible of exact measurement as is intensity to some degree. With respect to intensity, he approved the now familiar argument against Fechner that sensations show intensitive differences but are not measurable quantities.

He admitted that statistics have proved useful both in the psychological and in the esthetic field. The great amount of statistical work which had appeared in America, however, led him to remark that there has developed a statistical disease. After all, he said, Aristotle was correct when he stated that qualitative analysis was the chief function of psychology.

Stumpf ended his long introduction to the course with a list of recommended literature for beginners. Lotze's work was mentioned first and the *Medizinische Psychologie* was called a classic. Brentano's *Psychologie vom empirischen Standpunkte* was next mentioned with the

remark that it had been a source of much inspiration but that unfortunately only the first volume, which is of a very general nature, had appeared. Wundt's *Grundzüge der physiologischen Psychologie* gave a most detailed account of work in this field with, however, a predilection for work from the Leipzig laboratory and a not entirely accurate account of views from other places. Külpe's *Grundriss* was very solid. Jodl's *Lehrbuch* was not of the first rank, but Ebbinghaus's *Grundzüge* was to be most highly recommended. James's *Principles* was not a real textbook and so was more suitable for independent reflection. The works of Lipps, Cornelius, Röhmke, Höfler, and Höffding were too much influenced by the authors' own preconceived ideas to be recommended. Stumpf mentioned the following of his own publications: the *Raumvorstellung*, *Ton-Psychologie*, *Psychologie und Erkenntnisstheorie*, *Gemütsbewegungen*, and *Leib und Seele*.

The main part of the course consisted of a first part entitled "Individual Psychology and Psychophysics," while the second part, entitled "General Psychology and Psychophysics," contained only three chapters. Stumpf distinguished by these two headings between questions of concrete detail and of universal principles.

Part I

Chapter 1 of Part I, entitled "Anatomical-Physiological Introduction," was concerned with the nervous system and consisted of a brief description of the central and peripheral systems. Stumpf supposed that the nerve impulse was probably an electro-chemical process.

Chapter 2, which dealt with the sensations, started with a general description of sensation, perception, and apprehension (*Auffassung*). According to Stumpf, the term sensation is used in two different ways in the literature, as sensory content and as the psychic function of sensing. The description of our sensations is always a description of content. Sensations are the entire content in all its parts whether we are aware of these parts or not. Perception has to do with those parts of which we are aware. At any given moment there is only a small section of sensory content of which we are aware. As long as one does not hear the scratching of the bow in violin playing, for example, one has a pure tone sensation. The scratching is present in the mind as sensation but below the level of awareness or perception. When one becomes aware of the scratching, the perceived content resolves itself into two parts, tone and noise. In such an example there is no change in the material but only in the function of awareness.

Perception can be directed not only upon the parts of a sensory complex but upon the relationship between these parts. These relations are given with the sensations. They are not a product of the mind but belong to the material of our experience.

Apprehension is a judgment in relation to the content of perception. Perception can not be true or false. These characteristics belong solely to judgments and the judgments need not necessarily be expressed in words. Furthermore, apprehension may not be determined entirely by our perceptions. There are apprehensions, such as sensory judgments (*Abschätzungen*), which are based upon secondary criteria.

After these preliminary remarks regarding functions, the various senses were treated in an orthodox manner, anatomy and physiology coming first, then experimental material and finally theories. I shall, therefore, mention only those points which show Stumpf's attitude toward questions which were then in debate.

In regard to visual qualities, Stumpf emphasized the fact that both white and black are positive and simple visual sensations. His principal argument for black was that the darkest black is not obtained by closing the eyes but by contrast—not by absence of light but by the effect of light. He also stated that black is a more intense sensation than middle gray. Successive contrast or negative after-image raises the question as to whether such phenomena are merely illusions of judgment or real changes in sensation. The proof that they are the latter is given by the fact that if they were errors of judgment, they would disappear with concentrated attention. As they do not disappear, they must be due to changes in sensation. Stumpf took a similar position in regard to Helmholtz's explanation of simultaneous contrast through unconscious judgment. Stumpf believed that such phenomena are not due to illusions of judgment but to illusions of sensations. We not only think we see the contrast colors, we actually do see them.

Stumpf did not make a clearcut decision between the Helmholtz and the Hering theory of color vision. He remarked that for a long time Hering was victorious but, through the study of color-blindness, Helmholtz's theory had come to the fore again. He ended the discussion by mentioning the compromise which consisted of relating Helmholtz's theory to the peripheral process and Hering's to the central.

Stumpf was against Wundt's contention that the primary colors are based on physiological rather than psychological analysis. He conceded the point that violet is just as simple a color as the primary colors, but agreed with G. E. Müller that there are certain points in

the color series where changes are in the same direction, and other points where they are in different directions. At these latter points lie the primary colors.

Besides brightness, Stumpf admitted intensity as an attribute of color although, he said, this fact is difficult to demonstrate since intensity generally changes with brightness. In the black-white series, however, black and white are more intense than middle gray. It is, therefore, evident that the two attributes are independent variables. A second proof is based on the fact that images are always less intense than sensations. If weakening and darkening were the same function, then all our images of color would be very dark. This argument is an excellent example of the type of Stumpf's logical deductions.

In audition the first problem considered was in regard to noise and tone. Stumpf believed that there could be no noise that does not contain a tone but there can be tones without noises, as when we hear a tone from a distant horn on a still night. It is not correct, as Helmholtz stated, that noises are a number of variable tones. Noises are a unique quality of our auditory sense.

The simple tones form a series which parallel the vibration rate. This is a fact which, Stumpf believed, can not be explained. Mach tried to do so with his dull and bright elements, but experience does not verify his theory. Furthermore, we would be no better off since we would merely be substituting two series for one.

The intensity of tones increases with vibration rate and constant kinetic energy up to 3000 cycles. The volume also changes with the vibration rate. That children call tones large and small is not mere metaphor. The reason lies in the nature of the tone. Although it is not spatial quality as in vision, since we can not measure it, nevertheless there is something voluminous that differs from quality and intensity.

The very high tones have hardly any quality left. That is why there is no consciousness of intervals in this region. The upper limit of tone is 20,000 cycles but the upper limit of musical tone is only 5000 cycles. In answer to those persons who, on the basis of the differential limen, believe that the tonal series consists of discrete tones rather than a continuum, Stumpf said that the limen concerns the function of perceiving (*Wahrnehmung*) and not sensitivity (*Empfindlichkeit*). The continuum hypothesis can not be proven but it is quite probably true.

Tonal after-images exist but they are of very short duration and hard to demonstrate. It is owing to the short duration of the auditory after-

image that one can make such accurate temporal discriminations. Contrast does not exist in audition. A tone keeps its pitch and is not changed by another tone. An observed change is caused by a change of judgment, not of perception. If one listens carefully, one will recognize that the tone has really not changed.

That clangs are sharp when they have a large number of over-tones, Stumpf explained on the basis of tone-color. Even pure tones have a color consisting of pitch, intensity and volume. The last two attributes are particularly important. Since high single tones are sharp and loud, the clang color will be sharp when the chord contains many high partials, but the sharpness does not depend entirely upon the partials. A higher fundamental with the same number of partials as a lower fundamental is sharper than the latter.

Stumpf next asked whether it was possible to hear two tones strictly simultaneously. To answer the question, he employed his usual method of presenting all possibilities and then disproving all but one. He believed that we can actually hear more than one tone, although we do not always recognize them, since such recognition requires practice and attention as well as musical talent. Underlying this, as well as many other problems, is the difference between sensation and perception. Hearing of over-tones is particularly difficult, principally because of fusion, and that is why it was so long before overtones were discovered.

Helmholtz's theory of hearing is the only one Stumpf mentioned and he accepted it with reservations, but with a strong belief in the resonance principle.

As is to be expected, Stumpf devoted much time to the theory of consonance and dissonance. He presented all of his well-known arguments against Helmholtz, Lipps, Krueger, etc., and then described his own theory of fusion. He remarked that fusion belongs to the sensations just as does similarity. Fusion is a psychological fundamental fact. There must of course be a physiological cause of fusion but we do not yet know what it is, any more than we know the physiological cause of similarity. He stated, there was at least the advantage in the theory of fusion that it described the psychological facts. He considered the two main objections to the theory to be that it depends on the simultaneous hearing of two tones, while there is consonance of successive tones, and that it is in conflict with the theory of the origin of music, since primitive music was homophonic. Stumpf said in regard to the first objection that consonance between successive tones occurs

by means of memory images, and, in regard to the second objection, that in primitive music we often find the tones simultaneously presented, as when men and women sing together. On what the sensory pleasantness of consonance rests is not clear, but, he added, pleasantness has never been explained anywhere.

The rest of the senses Stumpf described in short order. Taste and smell received only a paragraph apiece. Temperature and passive touch (*Berührungssinn*) were treated together. The latter he differentiated in this place from active touch (*Tastsinn*) which he treated under a separate heading. Under movement or kinesthetic sensation Stumpf stated that Bain was wrong to call this sense the activity sense, since we have kinesthesis with involuntary as well as voluntary movement. Voluntary movement is complicated by the fact that we find here the will and the idea of result. These factors, however, are not new constituent parts of the sensations, as the advocates of the innervation feelings suppose. Stumpf felt that the innervation feelings played a rather unfortunate rôle in preceding decades and that James, Müller, and Schumann deserved much credit for their arguments against the theory.

The tactual sense (*Tastsinn*) is a combination of touch sensation (*Berührungsempfindung*) and muscle sensations, according to Stumpf. The sensations of resistance may be explained on the same sensory basis. Hard, soft, etc., are words which refer to a judgment concerning the nature of the object. He explained further that all of our sensations are closely connected with judgment about the object. This statement applies also to judgments concerning the number of objects. We may, for example, have many sensations from a single object held between several fingers and yet we perceive the many sensations as referring to the one object. Another example is the double sensation we seem to have when we touch a cane to the ground; namely, the contact between hand and cane and between cane and ground. Lotze made many deductions from such experiences and Weber used them in his theory of the external world. It will occur to the reader that Stumpf is here referring to what Titchener called the stimulus error.

In regard to the Weber circles, Stumpf suggested that the facts point to a physiological explanation. He was somewhat skeptical of the possibility of measuring fatigue by means of the two-point threshold but admitted that the experiments were still in their infancy.

Light and heavy are not new sensory qualities, according to Stumpf. There is the consciousness of the impulse and of the strength of the

expected muscle sensations and, with known objects, the added consciousness of the speed of movement.

At the time of these lectures, there was still doubt whether the semicircular canals were the organs of balance. Stumpf, however, believed they are the sensory organs but he did not think that under ordinary circumstances there were any conscious sensations from these organs.

Stumpf included the feeling-sense (*Gefühlssinn*) in this chapter on the senses. It has two qualities, agreeableness and disagreeableness, and included under this sense are pain, hunger, thirst, headache, toothache, nausea, shivering, fatigue, dizziness, and *Lust*, such as the *Lust* component of tickle or a good appetite. Stumpf believed that the feeling-sensations formed a separate class of sensory experience although they were closely related to the feelings. There were, then, for Stumpf eight senses: vision, audition, taste, smell, kinesthesis, touch (*Berührungssinn* —he did not here make the distinction which he previously made between *Berührungssinn* and *Tastsinn*), temperature, and feeling-sensations (*Gefühlsempfindungen*).

Stumpf discussed the possibility of all the senses having developed from one sense, a possibility which is more understandable on the physiological level than the psychological where one finds differences of genus. It is conceivable, he continued, that there may have been jumps in the psychological development at which points new sensations emerged.

The law of the specific energy of nerves of Johannes Müller was then described. The various phenomena which support the theory, such as the paradoxical cold sensation (paradoxical warm sensation had not yet appeared in the literature), and the numerous objections were discussed and the theory finally accepted.

Stumpf next devoted a very long section to the intensity of sensations and the psychophysical laws. At the outset he made it clear that when he dealt with intensity of sensation he referred to content and not to function. He then went exhaustively into the problem of the threshold. We must realize, he said, that up to the present we have been able to determine only the threshold of perception and not that of sensation, although it is possible that below this threshold there are sensations which we can not perceive even with concentrated attention. There then followed a description of the methods of determining the threshold and a detailed account of Weber's and of Fechner's laws, together with a criticism of the latter. Stumpf clearly recognized the weakness of

Fechner's theory of the relation of mind and body from the fact that one sensation can not be considered double the size of another. In his description of the various experiments which had been performed on the threshold, Stumpf did not fail to mention the experiment in Wundt's laboratory on tonal distance, which seemed to prove that equal sensation differences correspond to equal stimulus differences. The experiment, however, was worthless, he added, since not sufficient consideration was given to the facts of musical consciousness.

In a separate section concerned with the different ways in which sensations occur, Stumpf discussed after-sensations, synesthesia, hallucinations, and illusions. These he called subjective sensations and defined the term as meaning sensations which, ordinarily aroused by external stimuli, are now aroused by internal stimuli. Under after-sensations he considered of peculiar interest those long delayed after-sensations to which Henle first called attention and which are sometimes called sensory memories. Such after-sensations occur also in dreams. Under synesthesia (*Mitempfindungen*) Stumpf included such phenomena as sneezing when looking into the sun, hearing a tone when the outer ear is merely touched, and the filling in of the blind spot. This last phenomenon he classified under synesthesia within the same modality and explained it as central induction.

Hallucinations were placed by Stumpf under the category of sensations and were divided into voluntary and involuntary hallucinations. He cited the fact that Goethe could produce voluntarily an hallucination of flowers (eidetic imagery?) but added such voluntary hallucinations are rare. Hallucinations need not be completely sensory in character. There must, however, be a sensory core to which ideas may be associated and then apprehended as sensations. Since the difference between illusions and ordinary perceptions is one of grade rather than kind, he considered that illusions have no special psychological significance.

Chapter 3 was devoted to the difference between images (*sinnlichen Vorstellungen*) and sensations. Images contain in general the same content as sensations. They differ from sensations in regard to intensity and richness of detail. Intensity concerns the content and not the function; namely, what we have in consciousness and not consciousness itself. Stumpf stated categorically that the difference in intensity between the two is an absolute difference. An image is not only weaker than a corresponding sensation, it is weaker than any sensation.

Stumpf drew a sketch on the board at this point to show the break between the strongest images and the weakest sensations in the image-sensation 'continuum.' It was, however, characteristic of Stumpf to qualify this dogmatic assertion by stating that in *general* one can differentiate between an image and a sensation but that sometimes, as in dreams, this can not be done.

There are also several secondary differences. Images are more fleeting than sensations. Seldom indeed does an image last longer than a second unless we hold it with our attention, in which case it is quite possible that we are renewing the image rather than holding the same image. Another difference is that images may be voluntarily modified. Furthermore, the belief in the reality of sensations is a direct one, but in the case of images, if we should happen to have a direct belief in their reality, we would have at the same time a feeling that we should verify our belief. Lastly, images generally arouse a much weaker affective tone than do sensations with a similar content.

The intensity difference between different images is also a difference in content and not in function. If there is any difference of a functional nature, it is a difference of attention or effort. The function of the imagination is the same whether we have the image of a bright light or a weak glow.

Stumpf discussed the characteristics of clarity, distinctness, and vividness which Lotze attributed to images, and then criticized Herbart's psychology of the mechanics of ideas. Stumpf pointed out that the opposition of ideas, which Herbart supposed to be one of function, is actually a matter of content and when Herbart used logical contradiction as an example of his mechanics, he erred again, for here we have an opposition of judgments and not of ideas.

In Chapter 4 Stumpf described the phenomena and laws of memory under the two-fold division of primary and secondary memory. The primary memory image (a term originating with Exner) occurs immediately after the sensation and under normal conditions continues for a certain length of time. It is this fact that makes it possible for us to understand a spoken sentence or to perceive movement as movement. Stumpf disagreed with Schumann's contention that there need not be an actual continuation in primary memory of the content of perception but only an after-effect of such content. He also called attention to certain pathological cases where the primary memory is reduced almost to zero and described some of James's observations on this phenomenon.

Stumpf enumerated three forms of reproduction: (1) mechanical on the basis of association; (2) intellectual, on the basis of recognized relations; and (3) physiological, on the basis alone of brain processes. He then differentiated memory in the broad sense from recollection in which the memory is placed and dated, and recognition which requires a judgment that the content of the memory is a previous impression. Similarity and not identity is all that is necessary for such recognition, nor does the previous experience have to be present in consciousness for comparison with the memory. Fantasy is a combination of ideas that does not exist in nature.

If a perception or idea occurs in consciousness, there is a disposition for a similar idea to occur under similar mental conditions. Stumpf thus defined the law of association and reduced all other laws to this fundamental one. He then described the factors, such as repetition, which are conducive to correct mechanical reproductions. He spoke of a dominating idea which has a unifying effect on a sequence of ideas and this was the closest he came to describing the determining tendency. If there is a dominating idea without reflection, we have the pathological case of a 'fixed idea.' He also described the short-circuiting of associations, as for example in language where the meaning of a word is far removed from its original meaning, in art where we call a line graceful and in the training of animals (the process now termed conditioned response).

Stumpf's concept of intellectual reproduction corresponds to what is sometimes termed internal association. The two ideas are associated through their relation as parts of a whole and this relationship is recognized through a judgment. Some of the relationships which are effective are rhythms, tonal intervals, and similarity. In speaking of physiological reproductions, Stumpf warned against the too-ready acceptance of the theory that ideas may appear spontaneously (*freisteigenden*). He preferred to suppose that their arousal was caused by unconscious physiological processes. In cases where there is a break in consciousness, without an interference in the general course of thought, he believed we must also resort to such physiological processes as an explanation. After a description of the effect of bodily conditions on reproduction, Stumpf described the phenomena of amnesia, hyperamnesia, and paramnesia. Stumpf considered that in the case of paramnesia, a peculiar feeling-tone probably made a new impression seem familiar.

In his treatment of individual differences in reproduction, Stumpf

explained at some length why some persons can remember colors best and others tones. He thought that a finer sensitivity and an innate response to pleasantness or unpleasantness in a particular sensory field caused a heightened attention to such sensory content and thus increased their memory value. Stumpf also described the different types of memory, such as visual, etc., the individual differences in ability voluntarily to change the direction of the memory, the differences in the speed of memorizing and forgetting, individual differences in the amount of mechanical and intellectual reproduction, and finally the productive or fantasy type and the memory type. This last distinction led him to a discussion of geniuses in general and to outstanding memory artists in particular. Apropos of the genius Stumpf said that all great innovations in human existence have had their origin in the life of feeling. This is a surprising statement from one who had developed an otherwise rather intellectual system. In regard to gifted children, he remarked that it is difficult to say what is innate and what is learned, even when, for example, we know that an entire family is musical.

In his lengthy account of the newer experiments in memory Stumpf was concerned principally with the works of Ebbinghaus, Müller and Schumann, Müller and Pilzecker, and Steffens. He also described some of the applied work, such as Stern's experimental investigation of testimony and the attempt by psychiatrists to discover complexes in their patients by means of association and reproduction, attempts, Stumpf added, which are still in their initial stages.

In Chapter 5 on the psychology of space Stumpf first described the theories of space and then followed the method used in his early book on space to knock out the empirical theories, principally those of Helmholtz and Lotze, in order to establish his nativistic theory. Stumpf's intense interest in the topic led him to devote a relatively long time to it and to go into the subject in great detail. Lotze's theory he termed only half empirical, since Lotze agreed in general with Kant that space is an *a priori* form of perception. It is toward Lotze that Stumpf directed most of his fire. Helmholtz saw the difficulties of explaining local sign and stated that we should cease to try to explain them and merely accept them as a fact. This attitude Stumpf termed a retreat.

Stumpf in his positive arguments for a nativistic theory laid weight upon the fact that the optic nerve gives us colors with the attribute of extension as a unified impression. He thought that this fact alone makes

an empirical theory impossible. He added that it is not synthesis which is gradually learned but rather analysis.

When he came to speak of the physiology of space, Stumpf withdrew his former objections to Lotze's contention that there are physiological differences in the brain corresponding to differences in localization in space. Stumpf conjectured that separate parts of the brain determined the structure of the fibers. He thought, therefore, that there was probably a new specific energy which one might call "topogene" energy.

Stumpf described the empirical factors of space perception and agreed with the empiricists that they are of the greatest importance for the third dimension. He disagreed with Helmholtz that experience could not change true sensations. There are undoubtedly actual changes of sensations at times; at other times there is only a change of judgment. He then described reversible perspective which he considered a real change in sensory material. In regard to explanations of optical illusions, he stated that the nativists can use all the arguments of the empiricists and then one more, that of native endowment, and so can explain all of the spatial experiences. Stumpf showed a strong bias against the use of muscle sensations as an explanation of the experiences of visual space and remarked after reviewing the many theories of optical illusion that we have sufficient explanatory material without bringing them in.

Stumpf next described monocular and binocular vision and stated that the relief produced by the stereoscope was a matter of perception and not of judgment. That is to say, it is a real change of sensations, the proof being that, no matter how clearly we are aware that there are actually two objects, the unified perception does not change.

Against Darwin's contention that the nativists are correct for the individual's space perception, the empiricists for that of the race, Stumpf remarked that such a theory merely pushes the problem back and does not solve it.

After this long discussion of visual space, Stumpf treated the spatial qualities of the other senses very briefly. In touch there is a nativistic core but experience plays an important rôle. Stumpf agreed that in tactual space kinesthesis is a primary factor. In regard to the space perception of the blind, he did not admit that we are here dealing with a different space from that of a normal individual.

In auditory space the perception of distance and direction is dependent upon experience, but even in auditory space there is something nativis-

tic. That a tonal impression belongs to the right or left ear is a matter of the tonal sensation itself. Also one of the tonal attributes is extension.

Although there had been little experimentation on the spatial attribute of the other senses, Stumpf believed they all have some spatial quality.

The separation of our body from the outer world Stumpf agreed is entirely a matter of experience. We distinguish our own from foreign bodies because when we touch our bodies we have two sensations instead of one, and we learn to call those limbs our own which through the testimony of our own consciousness we are aware we have moved by our own will.

In the beginning of Chapter 6 on time, Stumpf stated that the root of the perception of time is the primary memory by which we become aware of a progressive change. As he previously remarked, it is through this primary memory that we are able to perceive movement. We also observe a progressive change by means of primary memory in the higher mental activities. It is thus directly through both internal and external experience that we obtain a concept of time. This statement is true, however, for only very short intervals. The secondary memories must be localized by means of the secondary criteria of experience, such as distinctness. Stumpf then described results of experiments in the judgment of time intervals and concluded that even relatively short intervals are judged partly on the basis of experience. Among such factors is probably the breathing rate, but Stumpf believed that Münsterberg exaggerated its importance. Pulse rate has hardly any influence. In general, one can say about our experience of time, as about space, that it is given directly with the sensations and is developed through numerous secondary factors.

In regard to the ability to wake at a definite time, Stumpf believed that general bodily feeling (*Gemeingefühl*) is probably the principal factor, although, he added, the problem has not yet been satisfactorily solved.

All intellectual activity consists in analysis and synthesis, Stumpf stated in Chapter 7 on synthesis (*Zusammanfassen*). Synthesis is even present in sensory impressions. He further stated that analysis must precede synthesis. We must differentiate before we can unify.

That we can perceive a series of tones as a melody, or a series of words as a sentence, depends upon the fact that we can grasp the series as a

whole. The question arises whether any change occurs in sensations and ideas when we group them, as for example in subjective rhythm. Stumpf found this a difficult problem, but he thought it possible that one could have such groupings without change in the individual units. A second question is whether an act of will is always necessary for such synthesis. This question he answered unhesitatingly in the negative. The first syntheses, as well as the first analyses, are forced upon us.

Stumpf also corrected what seemed to him false theories of synthesis. It is not true, he stated, (1) that synthesis is the only characteristic of the intellect, (2) that all the elements we can differentiate were originally isolated in consciousness or that all 'wholes' were originally isolated in consciousness, (3) that synthesis is identical with an act of will, (4) that there are different forms of synthesis which are original forms of experience (against Kant). Finally he repeated what he had previously said, that the mind never constructed relations, but that it became aware of the relations in the phenomena.

In introducing the problem of abstraction and generalization, the topic of Chapter 8, Stumpf stated that the origin of a concept is a matter of psychology but the meaning of the concept is not. He undoubtedly meant that psychology was concerned alone with the nature of the function of abstraction and generalization and that the meaning should be left to 'logology.'

Stumpf explained that an abstraction takes place when something is separated in thought which is not separated in the phenomenon. One of the examples he gave was a separation of function and content. To the assumption of certain psychologists that *change*, as for example the change of intensity from or to zero, which makes intensity stand out as an attribute, and *comparison according to similarity* constitute abstraction, Stumpf answered that change and comparison are conditions of abstraction but not abstraction itself. The concept derived through generalization is a unique mental construct.

Stumpf described the development of general ideas in the child and then asked whether in adult life particular ideas are necessary for thought. He answered that abstract thought has a certain independence of particular ideas, but that Aristotle was right in his assertion that one must from time to time return to experience in order to freshen the concept. He also explained this independence by stating that something new always occurs when there is a synthesis. This product of synthesis

he called *Inbegriff*. At this point he made reference to Von Ehrenfels' *Gestaltqualitäten*. In generalization the construct is the concept, in judgment it is the meaningful content (*Sachverhalt*). Even in the emotional functions we have similar constructs, such as value and good. Many of the general concepts are obtained, not as Locke maintained through reflection on the functions, but rather on the constructs, as for example the concepts of existence and possibility.

When Stumpf spoke of *a priori* and *a posteriori* judgments, he warned against confusing the problem of the origin of concepts with problems of epistemology. Furthermore, he stated that we must not confuse the origin of a concept with its objective reference. An innate concept can be objectively true. On the other hand, an empirical concept can be actually false.

Stumpf throughout the course frequently refuted the Darwinian theory as an explanation of the origin of various innate functions. He stated in regard to the topic of generalization that the only thing the theory could explain was that through the process of evolution abstraction and generalization had been increasingly easy to perform.

Chapter 9 was devoted to symbolic ideas. For Stumpf symbolic thought is especially necessary when a word means something that we can not actually imagine in its totality, as for example the word 'Berlin.' In general concepts we also have the same representative function of words. When images represent concepts, the former vary considerably for the same concept. Verbal symbols, as well as arithmetical symbols, stand with few exceptions for concepts and not for particular ideas. Conceptual thinking is independent of any fixed symbols, as exemplified in the case of Helen Keller. This fact is a proof that thought and speech are not identical. Stumpf also stated that symbolic thinking is not a new intellectual function, but is made possible through the function of generalization.

In the short Chapter 10 Stumpf discussed the ideas of psychic conditions (*von den Vorstellungen psychischer Zustände*). He stated that we can imagine not only external phenomena but also psychic functions. The basis of such a process is a sort of perception. We can direct our consciousness on the functions and notice them at the time or directly afterwards. It is also possible to have a memory image of a psychic function. However, it is only through reviving the function by reproducing the inducing circumstances that we can fully reproduce it in memory. It also frequently happens that there is nothing in memory

of the previous psychic function. All that is present is the external core, the phenomenon to which the function was related. Here, Stumpf said, we find the broadest use of symbols.

Chapter 11 was concerned with judgments. We have a judgment, according to Stumpf, when we speak of right or wrong in affirmation or negation. Even the expectation of a sensory phenomenon, as for example the expectation of thunder after lightning, is a judgment. Such a judgment, however, is elementary as contrasted with the higher or 'real' judgment.

Judgments can not be reduced to associations, nor are they merely the relation of two ideas. Rather a judgment is a new function. It has been thought that what is new in a judgment is more of an emotional than of an intellectual nature, but this is not true.

Stumpf then considered the intensity of judgment. He stated that we have a number of intensitive differences to keep apart, such as insight and blind judgments, probable and sure judgments, and wavering and stable judgments.

The section on the nature of the various types of judgment was partly psychology and partly logic. Of interest are Stumpf's remarks about blind judgments. Such judgments may occur when there is strong faith and lack of critical consciousness, as in early childhood, in dreams and in the case of primitive peoples. A second origin is the tendency to believe what is in line with the dominating feeling, such as optimism and pessimism. Finally there is the mere reproduction of a former judgment under the law of association. Most judgments, Stumpf added, are a mixture of insight and blind acceptance.

In Chapter 12 Stumpf discussed the feeling-sensations which he defined as a new class of sensory experience, which sometimes occurs independently and at other times in close association with other experience. He also discussed other theories of pain and *Lust* only to discard them in favor of his own theory of feeling-sensation.

Among Stumpf's many remarks about the feeling-sensations are the following: extreme heat and cold occur with pain sensations in which case we have two sensations. The same taste may have different feeling-sensations, although in such circumstances it may be that the taste itself has changed. It is possible that taste and smell sometimes occur without feeling-sensations. When the stimulus is very strong, it is easy to agree that there is pain as a separate experience. The difficulty lies in the medium intensities, but even here it is evident that the feeling-

sensations are fairly independent of the sensations with which they appear. The feeling-sensations of the lower senses have been very stable throughout the centuries, those of the higher senses very changeable. The image of a feeling-sensation readily goes over into an actual sensation. Feeling-sensations occur with many of the emotions. Certain feeling-sensations are inherited, such as the shudder at sight of a mouse or the sensory agreeableness of a musical interval. In reference to the latter example Stumpf stated that it must have taken only a few centuries for the pattern to have been stamped on the brain. The end-organs for pain are the free nerve endings, but the feeling-sensations of the higher senses must have a central origin.

The feeling-sensations may best be explained on the survival principle. Feelings of *Lust* are instincts that are connected with activities useful for survival, and sensations of pain are instincts connected with the harmful activities.

Chapter 13 was on the emotions which, according to Stumpf, are a new type of psychic activity. An intellectual function—a judgment—is the basis of every emotion such as joy or sorrow and the object of the emotion is either a known or a conjectured external or internal *Sachverhalt*.

Most of the chapter is concerned with a description of the James-Lange theory and the facts for and against the theory. In principle, Stumpf concluded, the theory must be rejected, yet there is much that is correct in it, especially the inclusion of sensory content in the emotional experience.

In Chapter 14 on the desires, Stumpf discussed at great length the wish, longing, demand and volition.[2] These mental activities, although closely allied to the emotions, constitute a new class of functions. The new element consists in the relation of the emotional states to something that we want or do not want to occur, or that we feel should occur, even though, as in some cases, we know it can not occur. In the instances where the event should or should not occur, there is the added factor of value.

In the desires, certainty of the result is not necessary but there must be subjective confidence. Through the addition of the intellectual function of expectation of the result, the emotional content is modified.

[2] Stumpf stated that he was using the verb *wollen* instead of the noun *Willen* to avoid hypostatization but later he used the two interchangeably.

In fact, the expectation gives the act a special tone which makes of it an act of will in the most pregnant sense. James's example of willing that a table should approach him can not be considered a fully developed will act, since this expectation is lacking.

There then followed a lengthy criticism of theories of the will, some of which, such as those of Schopenhaeur and Wundt, are too broad; others like that of the lawyer, Zittelmann, are too narrow. Stumpf's dualism came out plainly when, in discussing Zittelmann's theory, he stated that the will first affects the brain processes and these latter cause the movement. Stumpf was also opposed to the theories of Drobisch, Ribot, and Von Ehrenfels. All of these theories leave out the specific character of the will—that which James called the "fiat."

At the end of this chapter, Stumpf returned to a description of certain further characteristics of emotions which showed how closely allied he considered the emotional and conative states to be. In this place he spoke of habits in the emotional sphere and the development of secondary emotions, as for example when we love a person for another's sake. A secondary emotion may become a primary emotion through the disappearance of the judgment which aroused it, in which case we have an unreasonable passion such as avarice. At the same time, the intensity is increased through the disappearance of the regulating judgment.

Attention is the motor of our entire mental life, Stumpf declared in Chapter 15. For him attention equals interest and interest is defined as a joy in intellectual activity. Interest should be distinguished from the feeling of *Lust*, which is produced by the object itself. As an example of this difference, Stumpf remarked that a piece of music may be poor but interesting. Voluntary attention he defined as an act of will directed toward an intellectual activity. The factors most likely to attract attention are intensity and pleasantness and unpleasantness.

The direct effect of attention, Stumpf asserted, is greater *clearness* of the idea. This assertion follows from the more general one that all mental activities that occur with *Lust* occur more completely.

Stumpf then asked whether the attention could increase the intensity of a sensation and replied in the negative. The increasing attention on a diminuendo does not change it to a crescendo. One must not, in such cases, confuse clearness with intensity.

At the end of the chapter Stumpf admitted that kinesthesis is related to attention, but he added that muscle sensations are not identical with attention.

Involuntary movements were the subject of Chapter 16. Stumpf first discussed the view, prevalent at the time, that every sensation and idea is connected with demonstrable peripheral reactions. He cited Féré's theory of dynamogenesis in this connection and stated that as Féré used supersensitive subjects the law was not proven for normal subjects. He then described involuntary acts (1) that are produced by intense sensations or ideas; (2) that are expressive movements connected with emotion; and (3) that are acquired as habits during the lifetime of the individual. Finally he described the innate and acquired movements with special reference to Darwin's work on the expressive movement. Darwin's principle is generally being abandoned, he stated, although it may be true in certain respects.

Chapter 17 on the origin of acts of will was the last chapter of the first part of the course. Stumpf first enumerated two principles: (1) the innate connection between certain sensations and ideas, and movements; and (2) the intensifying of ideas through emotions. He mentioned the reflexes and then went almost immediately to what we would now call ideomotor action when he said that, if a feeling tone increases the intensity of the idea sufficiently, the idea will call forth a movement without the presence of a desire. This action is the root of desire in the sense of drive. Desire appears when an unpleasant sensation is present together with an idea of future agreeableness.

After a drive has frequently resulted in relief or satisfaction, the child becomes conscious of the relation between the two and thus learns to expect similar results under similar circumstances. The child soon learns to shift attention from result to antecedent. For example, it cries when it wants something. This experience, however, is not yet an act of will since no general concept is present but, outwardly, the situation looks like such an act. The general concept appears with the first sentences or their equivalent, and the child thus becomes conscious of the general idea that an agreeable experience may be obtained. The next step is the realization of values, such as good and bad, and finally there is an awareness of the concept of cause and effect. Stumpf added that it is through this inner perception that the idea of the causal law owes its origin.

Stumpf then discussed the various forms of choice between the means and between the ends, the development from mere association to rational thinking and the nature of internal acts of will where the goal is a mental state. Such internal acts, he stated, do not occur before

the third or the fourth year. He also described the various forms of retrogression from the fully voluntary act back to the involuntary act.

In regard to the freedom of the will, Stumpf maintained that psychologists must be determinists. Obviously ethical insight enters in, but it is a subject which should be left to ethics for consideration.

PART II

The second part of the course, as was previously mentioned, consisted of only three chapters. Chapter I on the classification of mental functions contained nothing of interest that had not been previously stated.

In Chapter 2 on the unity of consciousness, Stumpf took a position similar to that of James against the atomistic theory. The unity of consciousness is not a sum of parts but a totality, the parts of which are recognized only through abstraction.

In Chapter 3 Stumpf described the various forms of the unconscious. There are the unconscious links between conscious experiences, such as the dispositions, but whether they are mental or physical or psychophysical can not be answered. There are the unobserved contents of consciousness such as the unnoticed parts of our sensations. There are also contents of consciousness which are noticed but not further elaborated, as for example many of our sensations during sleep. These last phenomena, however, are not unconscious in the narrow sense of the term.

The question whether we can have unnoticed mental *functions* was the last topic which Stumpf treated in the course. After a survey of the pros and cons, he concluded that we are always conscious of such functions. The fact that the genius can not always describe his methods of work is not proof to the contrary.

Stumpf started his career under the influence of the old empirical psychology of speculation and intuition and, although he was never able to break entirely away from this influence, as is especially evident from some of his descriptions of the higher thought processes and the will, yet such lapses into the past are rare in comparison to his recognition of the value of observation and experimentation and his emphasis upon factual material rather than upon the result of speculation.

In regard to the contrast of function and content, Stumpf was not so successful in escaping from the influence of tradition. The old was still established with the beginning of the new and the two had not as yet become fully reconciled. It is true, as was stated in the beginning of

this paper, that the principles which Stumpf laid down in *Erscheinungen und psychische Funktionen* and *Zur Einteilung der Wissenschaften* had become a directive force in his thinking. It will have been noted, however, throughout these lectures how little Stumpf, the teacher, was actually guided by the psychology of Brentano. He anticipated in his functional psychology the objections to the atomistic psychology of content, but at this transitional stage in the history of psychology, he could not be expected to have pushed his theory very far. His teachings had to be a practical phenomenology.

In his introduction he discussed the measurement of psychic functions and on all occasions took pains to distinguish between function and content, but not only the lectures on the senses but even those on space, time, the emotions, and the higher processes, such as judgment, dealt for the most part with phenomena. In speaking of the intensity of sensations in relation to Weber's law and of the intensive difference between sensation and image, he took pains to make it clear that he was referring to content and not function. In fact, one of the few places where he emphasized function was in regard to the attribute of clearness as a function of the attentive process. Yet there is no doubt that Stumpf's insistence upon psychic functions as the subject matter of psychology helped to turn the science toward more objective research.

Another point of present interest is Stumpf's relation to Gestalt psychology. In 1907 Stumpf's students were beginning to discuss *Gestaltqualitäten*. Stumpf never took part in the development of the Gestalt theory nor can he rightly be said to have inspired it, but it will be recalled that he spoke in his lectures of the unitary nature of consciousness and the primacy of analysis over synthesis. He gave equal importance, however, to the synthesizing function of mind and the resulting constructs, and it is probably this view which prevented him from accepting the Gestalt doctrines.

As will have been recognized from this outline of his lectures, Stumpf never treated a subject superficially even when he addressed the beginning students. He was a great scholar, a keen observer, and an exact thinker. He was, above all, a man of vision and his opinions on important problems have greatly influenced the trend of modern psychology.

AFFECTIVE VALUE-DISTANCES AS A DETERMINANT OF ESTHETIC JUDGMENT-TIMES

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From everyday experience it would seem to be self-evident that it should take a person less time to choose between something he likes very much and something he likes very little than for him to choose between two things he likes almost equally well. A man's daily life would be intolerable if most of the alternatives presented to him were not such as almost to resolve themselves with a minimum of time and of effort; and it is when competing lines of choice not only are competing but also approach some equality in their valences that we have dilemma and drama. But now, supposing this general fact be granted, will it prove true within a very limited field of human interests, where the objects all fall within a narrow category and where the differences of affective value are likely to be slight? The present study is an attempt to obtain experimental verification of this point in the special field of esthetic choice.[1] *Does the time taken to make a choice vary consistently with the amount of difference in the affective values of the alternative stimulus-objects?*

The relevant literature is not voluminous. First it may be mentioned that in the field of psychophysics—where the affective judgment is not a primary consideration—a few studies (employing lifted-weights, light-intensities, sound-intensities, visual extents, or inclinations of lines) have shown greater or smaller relationships between the reaction-times of judging and other features of the judgments, as follows: (a) Time of judging is inversely related to the degree of subjective confidence experienced in making the judgment; the evidence appearing in comparisons between different categories of judgment (*e.g.* 'doubtful' as contrasted with 'greater,' 'less,' or 'equal').[2] (b) Times of judgments are inversely related to their correctness; so that the higher the percentage of correct judg-

[1] The writer gladly acknowledges the assistance of Miss Sybille Kahn Berwanger who carried on the experimentation and the tabulation of results.

[2] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 196–206; V. A. C. Henmon, The relation of the time of a judgment to its accuracy, *Psychol. Rev.*, 18, 1911, 186–201; S. S. George, Attitude in relation to the psychophysical judgment, this JOURNAL, 28, 1917, 1-37; J. Volkmann, The relation of the time of judgment to the certainty of judgment, *Psychol. Bull.*, 31, 1934, 672–673.

ments of a given pair, the shorter the average time.[3] (c) Times of judgments are inversely related to the objective distances between the stimulus-pairs; and curves of time when plotted analogously to the frequency curves of psychometrics are inversions of these curves.[4]

Is the principle (c) valid for another field of judgment, *i.e.* the hedonic? Explicitly, is the time required to make an affective choice related in any way to the affective-value distance between the stimulus-pair?

A few investigations have borne upon certain temporal aspects of affective reactions. Nakashima compared the time for arousal of affective with the time for arousal of sensory (cognitive) responses.[5] Using visual and auditory materials in series, he varied their presentation times, and then compared these times with the intensity of the affective experiences reported by his subjects. Although he found that the latter were unmediated and direct, they took longer times, the shortest time of necessary exposure being between 0.84 and 0.98 sec. In another study the same author used tactual stimulations, instructing his subjects to discriminate a sensory character as 'wet' or 'dry,' 'hot' or 'cold,' and the like, and also instructing them to discriminate the hedonic character.[6] Again, 'the important thing is that the affective averages are without exception the larger.'

Potter, Tuttle, and Washburn made a somewhat nearer approach to our problem.[7] They laid colors one at a time before subjects who were instructed to open their eyes at a signal and to judge each as 'indifferent,' 'pleasant,' 'unpleasant,' 'very pleasant,' and 'very unpleasant,' the time being taken by *E* with a stop-watch. Judgments of 'indifference' they found to take longer than judgments of 'pleasant' or 'unpleasant' (these two being equal), and the latter took longer than judgments of 'very pleasant' or 'very unpleasant' (these two also being equal). These judgments, it will be noted, are not discriminations between two stimuli, but reactions to one at a time.

A paper by Yokoyama comes still nearer our particular inquiry.[8] Incidentally to an experiment on 7 different colors on 7 different forms, he found that "the reaction-time for the comparisons of the most pleasant color-form with the 24 color-forms that come next in order of preference is greater than the average of the reaction-time for the comparisons of the most pleasant color-form with the 24 least pleasant color-forms." He offered the generalization that "the reactive times for affective judgments are . . . more rapid when the members compared differ widely in affective degree;" but no data is furnished beyond the above-quoted assertion concerning one extreme member and the two split-halves of the other 48 members. It is in order for us to inquire whether this relation holds in lesser degree for intermediate affective distances.

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[3] W. M. Kellogg, The time of judgment in psychometric measures, this JOURNAL, 43, 1931, 65–86; S. W. Fernberger and F. W. Irwin, Time relations for the different categories of judgment in the 'absolute method' in psychophysics, this JOURNAL, 44, 1932, 505–525; Einige zeitliche Bestimmungen der Urteile in der Methode der einzelnen Reize, *Arch. f. d. ges. Psychol.*, 89, 1933, 541–548.

[4] Kellogg, *op. cit.*; Fernberger and Irwin, *opp. citt.*

[5] T. Nakashima, Time-relations of the affective processes, *Psychol. Rev.*, 16, 1909, 303–339.

[6] Naskashima, Contributions to the study of the affective processes, this JOURNAL, 20, 1909, 157–193.

[7] H. M. Potter, R. Tuttle and M. F. Washburn, The speed of affective judgments, this JOURNAL, 25, 1914, 288–290.

[8] M. Yokoyama, Affective tendency as conditioned by color and form, this JOURNAL, 32, 1921, 81–107.

This question is recognized in a study of times of affective reactions (visual and olfactory) conducted by Wells,[9] but no clear data are furnished relevant to it.

A suggestion concerning method appears in a paper by Symmes,[10] but no data bearing directly upon our point accompany it. This author had her subjects sort picture cards, or colors, by the order of merit method; and she chose the median picture or color to serve as a standard. Then the subjects sorted the cards into two piles, those liked better than the standard and those liked less than it. Time having been taken for each sorting series as a whole, comparisons between series-members are impossible.

General Procedure

Stimuli and subjects. The stimulus-objects used in the present experiment were standard Milton-Bradley coated papers of the following saturated hues: red, green, blue, orange, yellow, purple, and orange-yellow shade no. 2. They were pasted in rectangles, 8 x 20 mm., in pairs on strips of gray cardboard cut to fit the Dodge tachistoscope. In general, the method of paired comparisons was followed. Every possible pairing of the colors was presented in the order suggested by Kowalewski;[11] then the pairs were presented again with right and left positions reversed to check space error and hand preference, making a total of 42 choices per experiment. Every *S* worked through three experiments. Nine undergraduate students of psychology (all men) served as *S*s.

Apparatus. The Dodge tachistoscope [12] was selected for presenting the paired colors. The tachistoscope was mounted on a platform at the level of *S*'s eyes. At the back was a specially built box containing a 60-watt daylight bulb which illuminated the apparatus. The box was grooved so that an aluminum screen could slide vertically between the light and the tachistoscope windows. Two openings were cut in the screen in such a way that (a) light entered the right window of the tachistoscope and illuminated the preëxposure fixation-card while the screen was being held in preëxposure position by a catch; and (b)—when the catch was released by a cord, over a pulley, which was attached to *E*'s key and the screen dropped—the second opening was brought into position and the stimulus-card was illuminated through the left window.

Reaction-times were taken electrically. *E*'s key, which mechanically operated the pulley-cord and catch and exposed the stimulus-card in the tachistoscope, by the same movement started an electrically operated stop-watch. *S* indicated his choice by pressing the right or left of two three-way keys on which his index fingers were resting. These keys were wired in such a way that either of them would stop the stop-watch and at the same time would light one of the two bulbs in the circuit, indicating to *E* whether a right or left choice had been made (cf. wiring diagram, Fig. 1).

Instructions and procedure. The following instructions were read to every *S* at the beginning of the first experimental period:

You are to look through the two openings in the box before you. You will see first a gray card with two dots. Fixate these. When I press my key, the dots will go away and two colors will appear. I want you to tell me which of these colors you prefer. Here are

[9] F. L. Wells, Reactions to visual stimuli in affective settings, *J. Exper. Psychol.*, 8, 1925, 64–76; and Reaction-times to affects accompanying smell stimuli, this Journal, 41, 1929, 83–86.

[10] E. F. Symmes, Aesthetic preference by comparison with standards, this Journal, 38, 1927, 432–435.

[11] Described in J. G. Beebe-Center, *Pleasantness and Unpleasantness*, 1932, 53 f., footnote 25.

[12] Raymond Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10–13.

two keys. Place the index finger of each hand on a key. You are to indicate which color you prefer by the key that you press. Press the key which is on the side of the color you prefer. When you make your choice, press the key firmly. It is not necessary to hit it. You must make a choice, but there is no hurry. This is *not* a reaction-time experiment.

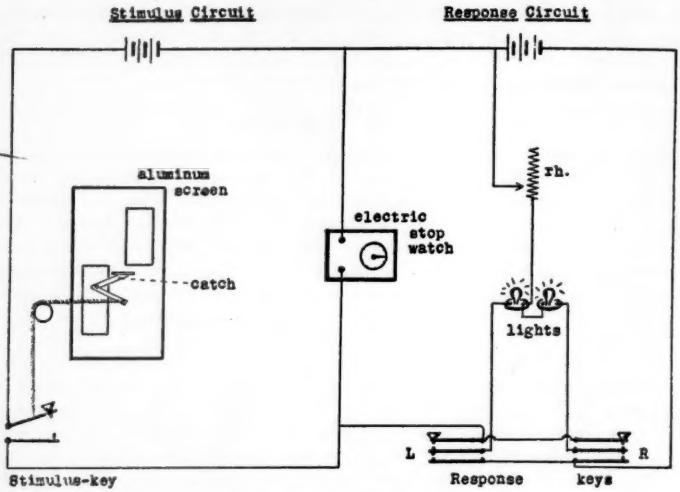

FIG. 1. WIRING DIAGRAM OF THE APPARATUS

At the beginning of each experiment, two sample cards were presented to *S*. When *E* said, "Ready," *S* fixated the crosses on the preëxposure card; and about 2 sec. later the stimulus-card was exposed. As soon as a choice had been made, the screen was re-set, the stimulus-card changed, and the procedure repeated.

Supplementary experiment. At the conclusion of the third experimental period, every *S* was presented with the 7 colors pasted individually on rectangles of gray cardboard, and was asked to rank them in order of preference, using the edge of the table as if it were a scale and placing the colors varying distances apart according to degrees of liking or disliking. The results of this order of merit experiment did not justify statistical analysis and are not included in the body of the paper, but they served a use in deciding cases of ties resulting from the method of paired comparisons.

RESULTS

Rankings of the colors. Table I gives, below "P-C," the rankings of the colors for the individual *S*s, obtained by computing the number of times each color was preferred in 126 comparisons.

From this table it may be observed that in general the *S*s tended to prefer blue and green and to discriminate against yellow and orange-yellow shade No. 2. This is in line with the findings from various

TABLE I

THE RANKINGS OF THE COLORS BY THE METHODS OF PAIRED COMPARISONS AND ORDER OF MERIT

Colors	Subjects																	
	J.B.		*L.K.*		*J.F.*		*G.S.*		*N.T.*		*R.B.*		*K.D.*		*C.K.*		*T.P.*	
	P–C	O–M	P–C	O–M	P–C	O–M	P–C	O–M	P–C	O–M	P–C	O–M	P–C	O–M	P–C	O–M	P–C	O–M
Blue	3	3	2	1	1.5	1	1	1	3	1	1.5	2	2	1	4	3	1	1
Green	1	1	1	2	3	5	5	6	2	2	5	5	3	3	3	4	2	2
Purple	5	5	4.5	3	1.5	3	2	2	6	6	3	3	4.5	5	1	1	4	4
Red	7	6	4.5	4	4	2	3	3	5	4	1.5	1	6	6	2	2	3	3
Orange	4	4	3	5	5	4	4	4	4	5	6	7	1	2	5	5	5	5
Yellow	2	2	7	7	6	6	7	7	1	3	7	6	4.5	4	6	6	6	6
Orange-yellow	6	7	6	6	7	7	6	5	7	7	4	4	7	7	7	7	7	7

TABLE II

AVERAGE CHOICE-TIMES OF THE COLORS OF EACH PREFERENCE RANK WHEN PAIRED WITH COLORS OF ALL OTHER RANKS

Ranks	Subjects									Mean	σ_{dis}	σ_M
	J.B.	*L.K.*	*J.F.*	*G.S.*	*N.T.*	*R.B.*	*K.D.*	*C.K.*	*T.P.*			
1st	2.03	1.96	1.20	1.34	1.31	1.24	1.12	1.22	1.06	1.39	.34	.11
2nd	2.40	2.01	1.26	1.87	1.34	1.28	1.11	1.45	1.34	1.56	.41	.14
3rd	2.52	2.37	1.23	2.26	1.28	1.34	1.22	1.45	2.14	1.76	.52	.17
4th	3.19	2.41	1.37	2.15	1.41	1.29	1.26	1.40	2.06	1.84	.63	.21
5th	2.99	2.43	1.42	2.22	1.43	1.37	1.34	1.43	2.08	1.86	.56	.19
6th	2.84	2.32	1.32	1.78	1.39	1.29	1.30	1.45	1.78	1.72	.51	.17
7th	2.61	2.05	1.37	1.60	1.35	1.31	1.03	1.38	1.55	1.58	.45	.15
Mean	2.65	2.23	1.31	1.89	1.36	1.30	1.20	1.39	1.71	1.67	.47	.16

studies of color preferences,[13] all of which are in agreement that the majority of adults elect blue as a first choice, red somewhat below, and yellow last.

The rankings of the different colors which were obtained by the method of order of merit (shown in Table I, opposite "O-M") conform still more to general findings. Although the rankings by the two

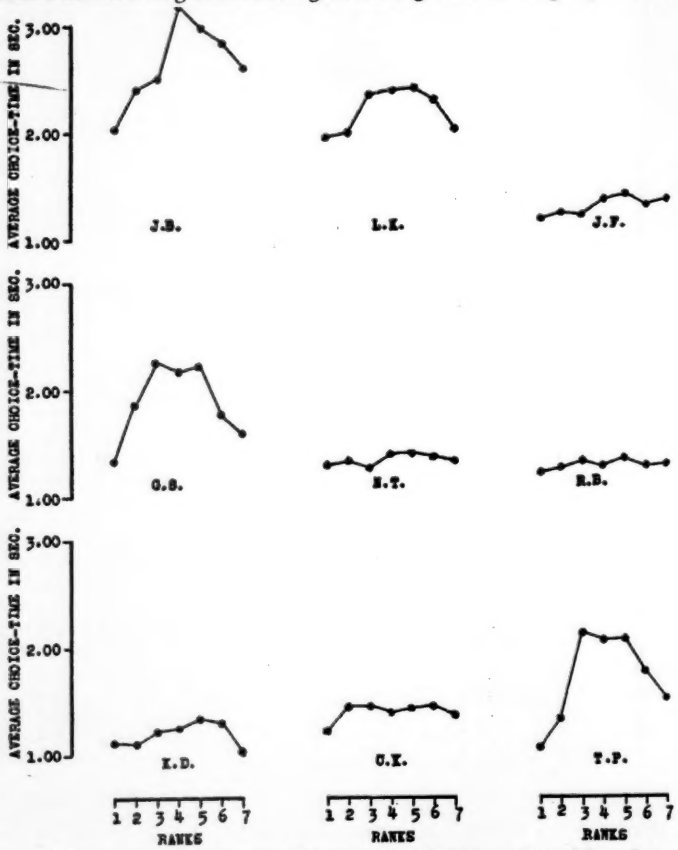

FIG. 2. AVERAGE CHOICE-TIMES OF THE COLORS OF EACH PREFERENCE RANK WHEN PAIRED WITH COLORS OF ALL OTHER RANKS
(From Table II)

[13] Jastrow, Winch, Aars, Dashiell, Garth, Michaels, Dorcus, Hurlock, and many others.

methods tend to agree in general (only one color was changed more than one place in rank), the results do not show the exact agreement founded by Barrett.[14] Perhaps the discrepancy in ranks was the result of the change in manner of presentation of the stimuli from the strictly controlled and formal choice-apparatus to the more informal use of a table, and also the change from electric to natural lighting. For the purposes of this experiment, it is felt that the method of paired comparisons gave a greater degree of quantitative exactitude than could have been obtained by the method of order of merit, rating scale of esthetic values, or any other method of getting at degree of esthetic feeling.

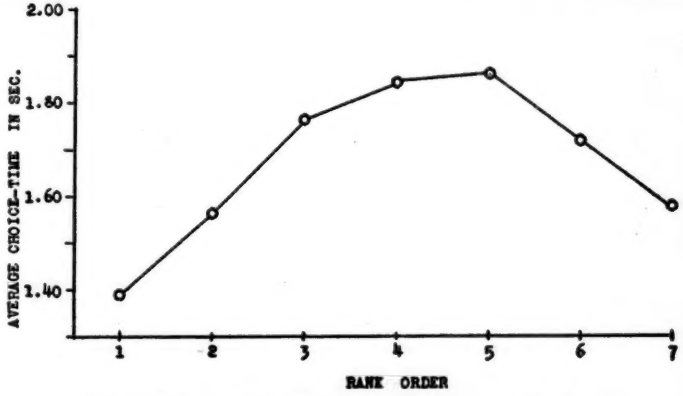

FIG. 3. AVERAGE FOR ALL *S*s OF ALL CHOICE-TIMES THAT INVOLVE COLORS OF EACH PREFERENCE RANK

For the sake of statistical simplification, in all subsequent tables no split ranks will be given. Whenever two colors receive the same ranking, the color which received the higher ranking by the order of merit method was considered the higher ranking color.

Choice-times arranged by final rankings of colors. Table II gives the average choice-times of the colors of each preference rank (as ultimately determined from the data) when paired with colors of all other ranks, for each subject. From this table, which is represented graphically in Fig. 2, one may see by inspection that when the color preferred most often (that is, the color ranked first), or the color preferred least often (ranked last), is presented with each of the other colors, the time of

[14] Mabel Barrett, A comparison of the order of merit method and the method of paired comparisons, *Psychol. Rev.*, 21, 1914, 278–294.

the *S*'s choice is shorter than for pairings of intermediate colors. This is more strikingly brought out by averaging the results for all 9 *S*s, and plotting as in Fig. 3. That the mean scores are fairly reliable appears from the low values of σ_M in the last column of Table II.

Choice-times between pairs of colors, arranged in order of preference. Table III gives a more detailed analysis of the data, showing for the individual *S*s the average time for choices between pairs of colors of the different preference ranks, arranged according to the intervals between the preference ranks. In Group I are shown the average choice-times for all pairs differing in one rank only. In the same way, Groups II, III, IV, V, and VI give the average choice-times for pairs of colors of successively greater difference intervals in preference rank. Fig. 4 is

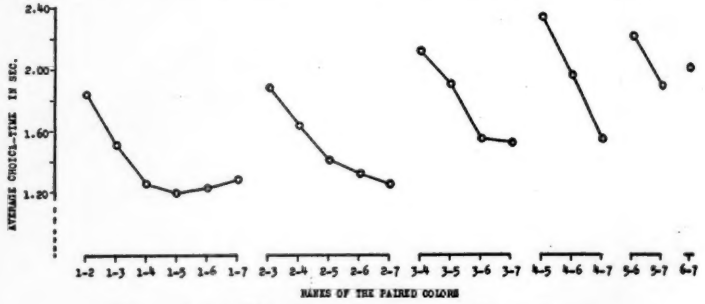

FIG. 4. AVERAGE TIMES FOR CHOICES BETWEEN PAIRED COLORS OF DIFFERENT PREFERENCE INTERVALS
(Average for all *S*s; from Table III)

plotted from the last column. It is brought out again very clearly that as the interval between the ranks of two colors—or, the difference in degree of preference for two colors—increases, the length of time to choose between them consistently decreases.

The reliability of the difference between Groups I and VI, that is, between pairs of colors differing only by one interval of preference and pairs differing by six, has been determined by Fisher's method for finding the reliability of the difference between the means of small samples.[15] Using his formulae, it was found that the probability that the difference between the average of Group I and that of Group VI—a difference of 0.795—could have occurred by chance is less than five percentage of the cases, and, therefore, the difference is a reliable one.

[15] R. A. Fisher, *Statistical Methods for Research Workers*, 1928, 105–112.

TABLE III

AVERAGE TIMES FOR CHOICES BETWEEN PAIRED COLORS, ARRANGED IN ORDER OF INTERVALS OF PREFERENCE RANK

Ranks of paired colors		Subjects								Mean
		J.B.	*L.K.*	*J.F.*	*G.S.*	*N.T.*	*R.B.*	*K.D.*	*C.K.*	*T.P.*	
I	1–2	4.40	2.27	1.25	1.21	2.01	1.37	1.20	1.24	1.61	1.84
	2–3	3.37	2.41	1.21	2.86	1.91	1.40	1.06	1.52	1.27	1.89
	3–4	3.26	2.69	1.27	3.08	1.14	1.56	1.48	1.42	3.29	2.13
	4–5	5.55	3.00	1.56	1.91	1.91	1.39	1.49	1.32	3.04	2.35
	5–6	3.59	3.17	1.57	2.70	1.85	1.58	1.66	1.75	2.06	2.22
	6–7	4.36	2.78	1.43	1.80	1.58	1.39	1.02	1.49	2.36	2.02
	Av.	4.09	2.72	1.38	2.26	1.73	1.45	1.32	1.46	2.27	2.075
II	1–3	2.30	2.30	1.15	1.61	1.51	1.17	1.01	1.34	1.20	1.51
	2–4	2.20	2.04	1.53	2.41	1.07	1.22	1.09	1.78	1.45	1.64
	3–5	2.07	2.93	1.20	2.71	.98	1.34	1.43	1.54	3.07	1.92
	4–6	4.28	2.95	1.25	1.62	1.65	1.18	1.41	1.30	2.05	1.97
	5–7	4.07	1.86	1.75	1.96	1.73	1.53	.96	1.49	1.74	1.90
	Av.	2.98	2.42	1.38	2.06	1.39	1.29	1.18	1.49	1.90	1.79
III	1–4	1.62	1.77	1.29	1.30	1.02	1.31	.97	1.18	.92	1.26
	2–5	1.38	2.05	1.15	1.84	.98	1.22	1.19	1.31	1.65	1.42
	3–6	1.90	1.70	1.24	1.75	1.01	1.30	1.26	1.55	2.31	1.56
	4–7	2.21	1.99	1.33	1.58	1.64	1.09	1.13	1.39	1.60	1.55
	Av.	1.78	1.88	1.25	1.62	1.16	1.23	1.14	1.36	1.62	1.45
IV	1–5	1.27	1.57	1.12	1.22	1.16	1.17	1.25	1.13	.92	1.20
	2–6	1.51	1.66	1.30	1.55	1.10	1.15	1.14	1.50	1.02	1.33
	3–7	2.22	2.21	1.33	1.53	1.15	1.23	1.08	1.30	1.69	1.53
	Av.	1.67	1.81	1.25	1.43	1.13	1.18	1.16	1.31	1.21	1.35
V	1–6	1.40	1.67	1.14	1.30	1.16	1.13	1.29	1.12	.86	1.23
	2–7	1.58	1.62	1.10	1.38	.96	1.33	.99	1.33	1.01	1.26
	Av.	1.49	1.645	1.12	1.34	1.06	1.23	1.14	1.225	.935	1.25
VI	1–7	1.20	2.20	1.27	1.38	1.03	1.26	1.01	1.28	.87	1.28

It is assumed that since the increase in reaction-time with each successive decrease in interval of difference between ranks is consistent with every *S*, the difference between each pair of intervals is also of significance.

By inspection of the averages assembled in Group I in Table III (1.84, 1.89, 2.13, 2.35, 2.22, 2.02) it appears that a choice involving pairs of colors which rank 3rd, 4th, and 5th in preference take longer than a choice involving ranks 1 and 2, or ranks 6 and 7. This is evidence of an even closer relationship between degree of preference and judgment time than was brought out in Table II.

Relation to psychophysical judgments. The evidence of this experiment, then, is that *the time taken to make a choice varies inversely as the amount of difference in the affective values of the alternative stimuli.* This appears to be (a) analogous to or (b) a special case of the finding in psychophysical work (stated at the first of this article) that times of judgment are inversely related to the objective distances between the stimulus-pairs. That it is (b), a special case of the latter, would be in line with a recent suggestion by Hunt and Volkmann, to the effect that "the similarity between certain phenomena in psychophysics and certain phenomena in affectivity is due to the operation of general principles of judgment in both fields." They go on to say that "many phenomena in the field of affectivity can be explained by invoking the principles of judgment rather than the principles of a unique affective process."[16]

So far as the present experimental problem and results are concerned, however, we are not warranted in accepting the last-quoted statement without qualification. Note that the stimulus-distances that affect judgment-time in a psychophysical experiment are purely physical values (*e.g.* 92 grm., 96 grm., 100 grm. weights), whereas those in an esthetic-choice experiment are subjectively established. We are reminded here also of the findings of a few investigators that the time for the very arousal of an affective experience as such varies with different individual unpaired stimuli.[17] It would, therefore, seem reasonable to consider that esthetic judgment-time is a function of affectivity both indirectly and directly. It is a function of it indirectly, in that *the affective reaction determines the relative values of the stimuli, the distances between which determine the judgment-time.* It is a function of it

[16] W. A. Hunt and J. Volkmann, The anchoring of an affective scale, this JOURNAL, 49, 1937, 92.

[17] See references above, especially those to Nakashima; Potter, Tuttle, and Washburn; and Wells.

directly, too, in that *the latent time of an affective reaction to a given stimulus taken alone varies with its place in a subjective scale of values.* For example, a color, tone, or other stimulus that evokes a hedonic response that places it, let us say, extremely high in an affective continuum, will tend to permit very rapid choosing responses between itself and other stimuli (which lie for the most part at a distance on the continuum); but it will also tend to arouse a direct affective response that is also rapid.

The close agreement of our findings to those of psychophysical experiments emphasizes the inter-stimulus-distance factor as an extremely important one in determining judgment-time. To what degree the affective latent time for the individual stimuli participates in this determination remains for further experimental analysis to answer.

Conclusions

(*1*) The results on the color preferences of the *S*s in this experiment agree in a general way with findings of other investigators. Blue received first place and unsaturated yellow-green seventh.

(*2*) When the average choice-times of the *individual colors of each preference rank, paired with colors of all other ranks,* are compared, it is found that there is a progressive increase in average length of choice-time from the first to the fifth rank and a progressive decrease from the fifth to the seventh rank. The choices involving colors at extremes of preference have shorter reaction-times than those involving colors of the middle preferences.

(*3*) When the average times for choices between paired colors are arranged in *order of size of the interval between the preference ranks of the colors,* there is again found a consistent decrease in choice-time as the difference between the ranks of the colors increases. The difference in choice-times between colors separated by only one interval of preference rank and colors separated by six intervals is a reliable difference. In general, then, it may be said that in choices between paired colors, the speed of judgment increases consistently as the degree of difference in preference between the colors, measured by number of choices for each, increases.

(*4*) These results on esthetic judgments conform well to the finding in psychophysics that the time for making a judgment is a function (inverse) of the distance between the stimuli to be judged. In esthetic judgments the stimulus-distances are, however, in turn functions of the affective reactions to the individual stimuli.

SOME MAJOR LAWS OF CONDUCT

By Edouard Claparède, Université de Genève
Editor, *Archives de Psychologie*
(Founded in 1901)

Conduct consists in purposive coördination of reactions.[2] It is an adaptation to immediate circumstances which implies functional unity in the individual concerned. Shall we be able, some day, to explain it mechanically? One should never say *ignorabimus*, but, even with help of biochemistry, it is improbable that we shall get so far. The biologist is quite unable, by means of biochemical formulae, to account for the *form* of the creature emerging from the egg of a hen or an ant. In the same way, such formulae cannot possibly account for the *form* of conduct, in other words for the mode of adaptation.

Each individual comes into the world with a tendency to develop and maintain a unified self. This tendency is analogous to that property of the egg-cell which makes it develop into an organism. In biology and psychology we must take this fundamental tendency for granted. That it is inexplicable in our existing knowledge of biochemistry, is no reason for ignoring its existence.

Although for the moment we are unable to establish mechanical laws of conduct, we can, however, formulate certain functional laws. Functional psychology is not opposed to structural psychology which seeks to establish causal laws. On the contrary, it renders it valuable assistance. By indicating the problems to be solved, functional psychology prepares the way for structural psychology. The dogmatic belief in the possibility of a mechanical explanation of conduct has hindered the recognition of its problems. We have here the main objection to primitive behavorism; it has tried to explain conduct while adopting, a priori, an attitude which is precisely one to blind it to the real problem of conduct. This is the problem, as I have said, of adaptations subordinated to the functional unity of the individual and the ends to which this leads.

Now the scientist must be relieved of all metaphysical and epistemological scruples. For him any principle or notion is legitimate,

[1] This article was translated by Dorothy Gandine-Stanton.

providing it is serviceable, convenient and productive. It matters little what these principles or notions uphold or contradict; whether they support this or that metaphysical creed, this or that epistemological conception. Their eligibility is determined only by their fertility or their utility. Now the functional point of view is convenient and productive and, among other things, it allows the formulation of certain major laws of conduct.

The functional standpoint is particularly valuable in the field of applied psychology. The educator or the psychotherapist, having certain ends in view, must seek means to attain these ends. Thus it is often profitable to consider psychological phenomena, not only in the sequence of cause and effect, but as of significance in determining the end. This means understanding them, but understanding psychological phenomena is, in effect, considering behavior from the functional point of view. Let us take, for example, the case of a child suffering from a feeling of inferiority which is made manifest in bragging or in mythomania. It is only if we consider these symtoms from the functional point of view that we shall understand their biological significance, that we shall see in them an attempt to compensate for a feeling of inferiority. If we keep to the causal point of view, we can note that an inferiority feeling results in mythomania, but never, from this point of view, can we understand *why* this cause produces this effect. Now it is just this *why* that is important for the educator in applying his treatment. It is the idea of compensation, which is a functional idea, which leads him to occupy the child in activities having the same functional compensatory value as the mythomania, without its moral and social disadvantages. Let us notice that the word "inferiority" is meaningless except from the functional point of view, for "inferior" means "inferior to something" but to what? Clearly it is to the ideals and ambitions which the individual finds inaccessible.

I wish to emphasize the fact that the adoption of the functional point of view does not involve adherence to any finalistic theory. To recognize the finality evident in psychological and biological phenomena is one thing; to seek to explain it by recourse to entelechies (such as the *psychoid* of Driesch or the *horme* of Monakow or McDougall) is another. Functional psychology does not propose to provide a final explanation of conduct and of mental activity. It seeks only to arrange their phenomena in a perspective more advantageous, and more nearly correct, than that which the causal point of view usually allows.

I am now going to expound thirteen functional laws of conduct

without claiming (in fact very much to the contrary) to give a complete list. These laws apply to the springs of conduct itself with all its forms, its meaning, and its modes of expression.

I. Laws Concerning the Springs of Conduct

(1) *The law of need.* The law of need may be stated as follows: *A need tends to evoke reactions proper to its satisfaction.* This law is fundamental. Every living organism is a system which tends to preserve itself intact. When its internal equilibrium is damaged, should the organism fail to make the necessary readjustments, it will disintegrate and die.

I have said that a need "tends to evoke" and not that it "evokes" reactions, for, in certain circumstances, a coexistent incompatible need may inhibit the reactions appropriate to the first need.

One can ask if the converse of the law is true. Is all activity evoked by need? In the present state of our knowledge it is hard to answer this question. There are activities produced by stimuli. Most frequently such stimuli are only effective when they coincide with a need. Without the need the stimuli are ineffective. External agents do not of themselves give rise to reactions. Walking in the street we are exposed to a great many light and sound waves, etc., which give no reaction at all. It is quite fair to admit that when one of these external agents really becomes an effective stimulus, it is because the organism has become sensitized to it, but what has aroused this sensitivity? It is a need, an actual or a latent need. Jennings has already said that "the reaction to a given external agent depends on the physiological state of the organism."[2] When we are hungry restaurants or food stores become for us effective stimuli, while they do not attract our attention when we have no need of food. We must, however, remember that reflex actions occur quite apart from any need, and it seems that certain spontaneous activity also emerges without reference to need. The experiments of Boldyreff, Richter and others have shown that, with rats, the periodic contractions of the stomach are related to phases of activity. It would appear that such contractions often occur before the animal is really in need of food. It seems as if there is an intermediate process inserted between the true need and the reaction, which functions periodically and autonomously, in response to a rhythm determined we know not how. An example of this internal autonomic rhythm is found in *Convolutae Roscoffenses* studied by Bohn. These are little worms which bury themselves in the sand at high tide and

[2] H. S. Jennings, *Behavior of the Lower Organisms*, 1906, 286.

reappear at low tide. Now this rhythm persists even in an aquarium where the creature is away from the impact of the waves.

Apart from these phenomena of internal rhythm in which activity can be induced independently of actual need, we can say that, in a general way, all activity (especially mental activity and the higher forms of conduct) is evoked by need.

(2) *The law of interest.* According to the law of interest *all conduct is dictated by interest.* In reality, this is the same as the law of need, for we are interested in that which concerns us at the moment, and we are concerned with that which can satisfy our needs. The law is here expressed in terms which correspond more adequately with psychological experience, for we usually seek, not to dispel a need, but to secure an object or a position which attracts us. Needs are in some way projected in to the external world and there transformed: they seem to us to be things to attain. The hungry man wants bread rather than the removal of his hunger.

We can, therefore, consider interest (as I said at the Congress of Psychology at Rome in 1905) as the "principe fondamental de l'activité mentale." When we seek to explain or justify our conduct at any given moment, we always come to a point beyond which we cannot proceed; we did it "parce que ça nous intéresse."

(3) *The law of momentary interest.* Every living being, according to the law of momentary interest, *acts in accordance with his dominant interest of the moment.* Or one could say: *The most imperative need of the moment dominates all others.* This law, which I formulated a long time ago,[3] expresses a well known fact. J. Locke has already said that "the greatest present uneasiness is the spur to action."[4] We see that the part played by instincts, tendencies, and needs (in determining which other tendencies should be superseded by any given tendency) is implicit in the Law of Momentary Interest.

(4) *The law of satiation and disgust.* Once a need is satisfied the reactions it evoked will cease *and the objects, which formerly met the need, will now arouse contrary reactions of repulsion and disgust.* This law of satiation

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[3] Edouard Claparède, Esquisse d'une théorie biologique du sommeil, *Arch. de Psychol.*, 4, 1905, 280.

[4] John Locke, *An Essay Concerning Human Understanding*, Bk II, Chap. 21, Sec. 40; J. Kollarits has studied the phenomenon of momentary interest among the nervous. *Das momentane Interesse bei nervösen und nicht nervösen Menschen, J. f. Psychol. u. Neurol.*, 21, 1915, 233-246.

regulates, not only organic needs, but mental needs as well.[5] It seems to be completely ignored by the majority of educators.

(5) *The law of the extension of mental life.* Mental life develops *in proportion to the size of the gap between needs and their means of satisfaction.* The law of need, mentioned above, is not, properly speaking, a psychological law since some needs (such as breathing and all automatic organic functions, as well as the regulation of vegetative functions of which we are unaware) can be satisfied without the intervention of mental activity.

When is it then that needs affects mental life? It is when satisfaction of need is not automatic. Mental activity is then required to supplement the insufficiency of the automatic adaptation of the organism.

(6) *The law of awareness.* The sixth law is: *awareness of a process, relation, or object, is delayed in proportion to the earliness and length of the period in which its automatic unconscious use has been implied in conduct.* This is a corollary of (5). I was led to enunciate this law after observing that children of six to seven years of age, being questioned about differences or resemblances existing between familiar objects, find it hard to indicate resemblances while they easily find differences.[6] Nevertheless, very young children behave as if they are aware of resemblances; a baby will call all men "Dadda" and will give the same name to all animals. How can we explain this paradox? Clearly it is due to the fact that children, having an automatic tendency to deal similarly with all things which satisfy any one need, do not become conscious of the similarity of which they have implicity and unconsciously made use. Piaget found this law operative in the development of child thought. A child, for example, is often quite unable to define a word he knows well, "il agit sa définition avant d'en avoir pris conscience."[7]

This law has as corollary *the law of automatization* or *of custom: When any adaptation can be correctly made without conscious effort or awareness, the behavior involved becomes automatic and unconscious.*

(7) *The law of anticipation. Any need which, by its very nature, is unlikely to be satisfied immediately appears early.* Observation shows that, in practice, need or interest is experienced before the moment when life

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[5] Cf. the interesting article of Lewin, *Die Bedeutung der psychischen Sättigung für einige Probleme der Psychotechnik, Psychotechn. Zsch.*, 5, 1928, 182-188.

[6] Cf. Edouard Claparède, La conscience de la ressemblance et de la différence chez l'enfant, *Arch. de psychol.*, 17, 1918, 67-78.

[7] Jean Piaget, *Le jugement et le raisonnement chez l'enfant*, 1924, 280.

is endangered. We feel hungry long before we are likely to die of starvation; we fall asleep a certain time before we are intoxicated or asphyxiated. We might almost say that we eat two or three weeks too soon, that we sleep three or four days too soon! It is very easy to understand the function of this margin which exists between the subjective perception of need and the onset of the objective organic need, a margin which ensures that the individual shall not be caught unawares. If needs for which satisfaction must be sought emerged only at the last minute, the individual would inevitably be lost. He could not in this last minute obtain the things required for the recovery of his organic equilibrium. It can thus be seen that the law of anticipation is implied in the law of the extension of mental life.

This phenomenon of anticipation leads us to distinguish two zones of behavior. There is a deep zone, that of *endogenous need*, and a more superficial zone of exogenous need which might be called the *zone of appetite*. The sight of a beautiful fruit may make us want to eat when we are not hungry, or we may wish to drink when we see a glass of port wine although we are not thirsty. It is the additional intensity of anticipation which distinguishes appetite from need. Appetite has considerable biological value, a precautionary value: it enables us to benefit by the use of certain foods and drinks when they do not respond to any existing need.

These two zones exist also for psychological needs. Apart from real or endogenous need, as in the solution of some problem in order to free the individual from some difficult situation, there is also intellectual appetite aroused by objects and external situations which excite our curiosity. Intellectual appetite is characterized by the fact that we have no immediate need of the knowledge we seek: it operates independently of the demands of our present conduct: it prepares for possible conduct of the future. To be curious is to desire to know in advance of a situation which may perhaps never arise.

Intellectual activity is thus largely engaged as an outer defence in the economy of the organism. Mental life has interests which are not concerned with the organic needs of the moment. While remaining related to the zone of endogeneous need, it has a certain autonomy within the sphere of its needs (or its appetites).

II. Laws Concerning Execution of Action

(*8*) *The law of reproduction of the similar.* A need tends to reproduce reactions which have formerly proved desirable; *it tends to evoke again*

conduct which has already succeeded in similar circumstances. This is a law of considerable importance. It brings out the functional value of memory, habit, and past experience in general, in determining our present mode of adaptation.

It is hardly necessary to give examples. We will observe only that the tendency to reproduce the similar is often so strong that it overreaches its aim and causes errors and misunderstandings. A child who has been bitten by a dog may believe that all dogs bite. Reasoning by analogy and induction also illustrate the application of this law.

What is the mechanism which ensures this reproduction of the similar? Assuredly that is a difficult question. Association will not explain it. I think we must go to a process which seems essential in all adaptive behavior, and without which we should not know how to profit by experience; I refer to the process of *implication*. When a child has been burned by a fire, fire will imply, for him, the process of being burned. Implication is distinct from association. In association of objects (or their images) the interconnections are determined in a purely objective way—as by contiguity or concrete similarity—and have nothing to do with the needs or interests of the individual. On the contrary, the interconnections involved in the process of implication are created and reproduced under the guidance of need. In implication an object, or idea, will indicate an implied object or idea. It will herald its arrival. The implied idea is evoked, not in a sequence of association by contiguity or similarity, but in accord with its functional value in the situation concerned. If a thirsty dog drinks at a fountain, he will return to the fountain when he is next thirsty. This similar behavior occurs again, not through simple contiguity of the sensation of thirst and the image of the fountain, but on account of a dynamic relationship between thirst and the fountain. The fountain is a means of satisfying a need and has, for this need, a functional value.

This law of the reproduction of the similar is effective at all levels of mental activity from simple conditioned reflexes to the highest type of creative thought.

This process of implication by which two phenomena are thought of as being *necessarily* related is, in my opinion, a *primitive* tendency.[8] I mean that this feeling of necessity does not result from the accumulation of experience. Experience serves only to destroy implication

[8] Cf. Ed. Claparède, La genèse de l'hypothèse: Étude expérimentale, *Arch. de psychol.*, 24, 1933, 102 ff.

should it prove to be unjustified. Thus, the child who believes, after his first unhappy experience, that all dogs bite those who approach them, will learn from experience that this is not the case.

(*9*) *The law of trial and error* (*tâtonnement*). In a situation so strange that earlier memories are not aroused, and the law of the reproduction of the similar cannot operate, *a need will give rise to a series of exploratory reactions, of trial reactions, of gropings* (*tâtonnement*). I shall say little about this phenomenon which has been brought out particularly well by Jennings, and also by Morgan. For my part, I have sought to show that trial and error occurs in the working of intelligence, which is also linked with the most primitive behavior.[9]

(*10*) *The law of economy of effort.* A need tends to seek satisfaction along the line of *least resistance.* I will restrict myself to the bare mention of this law which everyday life will verify for us daily. I would like, however, to refer to its experimental verification in Loh Seng Tsaï's work with rats. When the path to the desired food was blocked by different obstacles, the one tackled was always that which could be most easily overcome.[10]

(*11*) *The law of substitution.* When an end cannot be reached by one kind of behavior or technique, *another kind of behavior or technique, likely to lead to the same end, is substituted, or tends to be substituted.* This is a general law which takes at least five different forms, all having the same functional significance. They all serve to reach an end and satisfy a need by means other than those which have proved ineffective.

(*a*) *Simple substitution.* When any technique proves inadequate, either momentarily or completely, *it tends to be replaced by other techniques.*

A starfish, turned on its back, prefers to right itself by using the arms nearest to the madreporic plaque; if movement of these limbs is prevented, the animal will use others. A schoolboy, who does not understand the proof of a geometrical theorem, will learn it by heart. He lets memory replace intelligence. Conversely, intelligence can replace memory; the proof may be forgotten and reconstructed by reasoning. Such simple substitution is also constantly being found in the realm of pathology after cerebral lesions, amputations, and so on.

(*b*) *Compensation.* When the disturbed equilibrium cannot be reëstablished by adequate reactions, *compensation is effected by reactions antagonistic to the discrepancy which exists.*

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[9] Ed. Claparède, Tierpsychologie, in *Handwörterbuch der Naturwissenschaften*, 1913; and La psychologie de l'intelligence, *Scientia*, 22, 1917, 353-368. On the "tâtonnement dirigé," see La genèse de l'hypothèse, *Arch. de psychol.*, 24, 1933, 1-155.

[10] Loh Seng Tsaï, The law of minimum effort and maximum satisfaction in animal behavior, *Monog. Nat. Res. Instit. Psychol., Acad. Sinica*, 1, 1932, 1-47. Tsaï has also formulated a *law of maximum satisfaction:* An animal will choose the path leading to the greatest satisfaction of need; a thirsty rat, which is not hungry, placed in front of food and water, will take water first. This law corresponds to our law of momentary interest.

Everyone knows how large a part compensation plays in mental life. Flournoy has already shown clearly the compensatory function of somnambulistic wanderings;[11] Freud has noted it in dreams and numerous obsessions; Ribot finds it in affective thought;[12] while the work of Alfred Adler has done most to make the idea of compensation enter into the minds of child psychologists and teachers. Probably many childish games serve to compensate for the child's natural feeling of inferiority, and prevent him from suffering on this account.[13]

(*c*) *Symbolic derivation.* When external circumstances or internal conflicts prevent objective satisfaction of a need, *satisfaction is sought through lateral reactions which act as symbols for the real means of satisfaction.*

It is enough here to give play as an example. In play, one asks that fiction shall supply satisfaction to those tendencies which cannot, in real life, be allowed full development. We might also mention the many derivations and sublimations described by the psychoanalysts.

(*d*) *Regression.* When a technique belonging to a certain psychophysiological level proves impracticable, *the individual resorts to a technique of a lower level.*

When one cannot solve a puzzle by reasoning, one tries more or less haphazard trial and error—an inferior mode of reaction. In sleep, when higher mental processes are inhibited, inferior modes of thought are manifest in dreams. The reasoning of the schizophrenic has often been likened to archaic thought. I, myself, in 1905, considered certain hysterical symptoms as regressions to an earlier phylogenetic state.[14]

This law of regression has been expounded by different writers: Hughlings Jackson found it in aphasia; Pierre Janet in psychasthenia; Von Monakow, Head, Goldstein and others have drawn attention to the functional hierarchy which is our psychophysical structure, and to the fact that, when the higher levels of this structure are inaccessible, behaviour is in accordance with some lower level. It is this that Goldstein has called the *Hierarchie des Abbaus*. Flournoy has, for his part, pointed out the infantile nature of the productions of somnambulists and mediums.

(*e*) *Progression.* When an individual has been at first tempted by the law of economy of effort to seek his end by recourse to a technique of low level and has failed, *he will next try a technique of higher level.*

I think this law can be enunciated, although the truths it expresses have, as yet, hardly been noted in any exact experiments. Constantly one finds, in everyday life, the following sequence; instead of thinking, one starts with guessing (tâtonnement) and, only if that is unsuccessful does one begin to think. I have verified this in my experiments on thought; it is what I have called "tâtonnement de la paresse."[15] Rey found the same phenomena with children; he calls this "genetic recapitulation."[16] Confronted with a novel task, a

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[11] Th. Flournoy, *Des Indes à la planète Mars: Étude sur un cas de somnambulisme avec glossolalie*, 1900, 1-420.

[12] Th. Ribot, *La logique des sentiments*, 1905, 115.

[13] Ed. Claparède, Le sentiment d'infériorité chez l'enfant, *Cahiers de pédag. exp. et de psychol. de l'enfant*, 1, 1934, 1-25.

[14] Cf. Ed. Claparède, Esquisse d'une théorie biologique du sommeil, *Arch. de psychol.*, 4, 1904, 245-349; Quelques mots sur la définition de l'hystérie, *ibid.*, 7, 1907, 169-193; E. Kraepelin, Über Hysterie, *Zsch. f. d. ges. Neur. u. Psychiat.*, 18, 1913, 261-280; W. H. R. Rivers, *Instinct and the unconscious: A contribution to the biological theory of the psycho-neuroses*, 1920, 1-252; E. Kretschmer, *Über Hysterie*, 1923, 1-115.

[15] Cf. Claparède, La genèse de l'hypothèse, *op. cit.*, 124.

[16] André Rey, *L'intelligence pratique chez l'enfant*, 1935, 219.

child will tackle it in a way characteristic of a much younger child and will gradually improve his technique until he reaches the normal for his age.

(12) *The law of subjective dominance.* The self tends to subordinate the facts of reality to its own inclinations, aspirations and needs. It tries to *ignore reality, to deny it whenever it is in opposition to the desires of the self.* This law guides all the affective logic which is found so frequently in both public and private life. "L'humanité," Amiel has said, "tient peu à l'exactitude et beaucoup à la grandeur. Ce qui lui est cher, c'est son rêve et son espérance, c'est-à-dire son idéal et non la réalité. L'homme se defend tant qu'il peut contre le vrai, comme un enfant contre une médecine." Stuart Mill too says that if geometric truths had troubled men, they would long ago have found it false. It is subjective dominance which leads to such phenomena as repression, rationalization, self-righteousness, and autistic thinking.[17]

(13) *The law of functional autonomy.* At any moment, throughout his development, a living being is a functional unity. *His capacity for reaction is adjusted to his needs.* This functional autonomy has been ignored for a long time. Psychologists and educators have long continued to consider the child as an imcomplete adult, *an homunculus.* Rousseau was the first, as I have previously tried to show,[18] who pointed out that a child is nothing of the sort. "Chaque âge, chaque état de la vie," he has said, "a sa perfection convenable, sa sorte de maturité qui lui est propre. Nous avons souvent ouï parler d'un homme fait; mais considérons un enfant fait: ce sera plus nouveau pour nous et ne sera pas moins agréable." In spite of Rousseau, it was not until the beginning of this century that we began to rediscover, largely through the work of Dewey, that a child is a "living unity."[19]

It is this phenomenon of functional unity and harmony that I have called "functional autonomy." We all know how the rediscovery of this principal of autonomy has revolutionised educational theory and practice. Education is no longer only a preparation for life, it is a life in itself.[20]

Conclusion

It is obvious that the preceding pages do not claim to present any new facts. They have been written only with the thought that it

[17] Ed. Claparède, L'auto-justification, *Arch. de psychol.*, 20, 1927, 265-298.

[18] Cf. Claparède, J. J. Rousseau et la conception fonctionnelle de l'enfance, *Rev. de mét. et de mor.*, 20, 1912, 391-416.

[19] Quoted from I. King, *The Psychology of Child Development*, 1903, 9.

[20] Cf. on this topic Claparède, *L'éducation fonctionnelle*, 1931, 236.

would be of value to summarise, in the form of laws, some of our knowledge about conduct. Psychology is often criticized as being still unfit to be called a science. A science centers round a system of laws, which form, so to speak, the skeleton of the science. I have thus thought it useful, not only from the practical point of view, nor from didactic considerations, but also for its theoretic importance, to give psychology, by the formation of certain laws, the back-bone it needs. Without it our science will appear to remain, quite wrongly, in an embryonic and quasi-protoplasmic condition.

DIE UNMITTELBARE TEILUNG EINER GEGEBENEN ZEITSTRECKE

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(Founded in 1903)

Das American Journal schuf bereits bei seiner Gründung eine internationale Arbeitsgemeinschaft der Psychologen. Denn seine Amerikanischen Leiter, Stanley Hall, Sanford und Titchener, deren wir bei diesem Jubiläum ihres Journales in Verehrung gedenken, nannten als Mitarbeiter auf dem Titel auch ihre deutschen Studienfreunde aus der Wundt-Schule Kirschmann, Külpe und Störring, der noch heute, wie einst auch die beiden anderen, dem aus Wundt's *Philosophischen Studien* hervorgegangenen "*Archiv für die gesamte Psychologie*" als Mitherausgeber angehört. Auch waren sie noch lange Zeit Mitglieder des "International board of coöperative editors," wie diese Arbeitsgemeinschaft später auf dem Titel kurz bezeichnet wurde. Gerne habe ich deshalb der ehrenvollen Aufforderung der jetzigen Herrn Herausgeber Folge geleistet, diesem Fest-Bande eine kleine Studie beizusteuern, und ein experimentelles Thema gewählt, das zur Analyse der Zeitvorstellungen gehört, in der schon die ersten Bände des *American Journal* durch die wertvollen Abhandlungen Sanfords und Boltons, sowie Titcheners und seiner Schüler der deutschen Psychologie reiche Anregung zurückgaben.

In meiner Darstellung der Bewusstseins-Phänomene der Zeitvorstellung vor etwa dreissig Jahren habe ich die Möglichkeit, eine gegebene Zeitstrecke ohne weiteres nach dem subjektiven Eindruck in n objektiv annähernd gleiche Teile zu zerlegen,[1] gegen die Behauptung Meumanns angeführt,[2] dass die Zeitgrösse "absolut unanschaulich" sei. Meumann vertrat zwar selbst gegen F. Schumann die Ansicht, dass sich die Zeitvergleichung auf einen besonderen Bewusstseinsinhalt des Zeitlichen stütze, und Wundt sagte wenigstens von den kurzen Zeiten, deren Anfang bei ihrem Ende noch im Bewusstsein ist, dass sie ähnlich vorgestellt würden, "wie etwa das Verhältnis der Teile einer eingeteilten Linie von

[1] Wilhelm Wirth, *Die experimentelle Analyse der Bewusstseinsphänomene*, 1908, 296.

[2] Ernst Meumann, Untersuchungen zur Psychologie und Ästhetik des Rhythmus, *Philos. Stud.*, 10, 1894, 249-322, 393-430 (S. 314).

mässiger Ausdehnung Gegenstand des unmittelbaren Augenmasses ist."[3] Um so mehr muss es überraschen, dass sich jene Behauptung Meumann's gerade auf diese kürzesten Zeiten bezog, also auf Zeitintervalle von 0,3 bis 0,5 sec. "innerhalb der Grenzen der genauesten Zeitschätzung. Als Kriterium ihrer "Anschaulichkeit" betrachtete er nämlich eine einigermassen genaue Lösung der Aufgabe, zu einer in Pausen von 2 sec. mehrmals wiederholten leeren "Normalstrecke" *N*, die mittels des Wundt'schen Zeitsinn-Apparates durch zwei Schläge eines elektromagnetischen Schallhammers begrenzt war, einen bestimmten Bruchteil $(1/n)$ *N*, also die Hälfte, das Drittel u.s.w. zu finden. Als Versuchspersonen wählte er mehrere im Klavierspiel geübte Musiker. Diese hatten aber in den zuerst beschriebenen Versuchen diesen Bruchteil nicht einfach nachzutaktieren, sondern die Kontakte am Apparat einzustellen, bis ihnen die passiv gehörte Strecke dem gedachten Bruchteil gleich erschien. Meumann schildert das ganz unbefriedigende Ergebnis folgendermassen:

> Die Hälfte des Normalintervalles herzustellen, ist schon schwierig und leidet unter den aus den Zeitsinnversuchen bekannten Fehlern. Das Drittel und Viertel wird gewöhnlich zu klein, manchmal aber auch zu gross gemacht, das Viertel nicht selten grösser als das Drittel. Dabei hat der Beobachter das Gefühl absoluter Unsicherheit, er ist sich nur bewusst, überhaupt einen recht grossen Unterschied einzustellen. Von einem Projizieren bestimmter Teile der Zeitstrecken auf einander kann keine Rede sein, das absolut Unanschauliche der Zeitgrösse kann kaum besser deutlich gemacht werden wie in diesem Versuch.[4]

Bei der Relativität des Begriffes der "Anschaulichkeit" sollte man aber mit der Behauptung ihres völligen Fehlens vorsichtig sein. Auch bei *Raum*-Teilungen findet man unter analogen Bedingungen ähnlich schlechte Leistungen, wenn sich die Normal-Länge und der zu beurteilende Bruchteil *in die Tiefe* erstrecken, deren Wahrnehmung auch sonst mit der "Zeit-Perspektive" verwandt ist, ohne dass man ihr alle Anschaulichkeit absprechen könnte. In einer Reihe von 10 Versuchen setzte ich auf einem fast in der Meridian-Ebene horizontal gespannten Faden vier Marken *a*, *b*, *c*, *d*, hinter einander, von denen die Lage der mir zunächst stehenden *a*, *b*, *c* von einem Gehilfen für mich unbekannt eingestellt wurden, während ich selbst die Lage von *d* suchte, durch die die Strecke *c-d* ein bestimmter Bruchteil von *a-b* werde. Hierbei erhielt ich folgende subjektiv ganz unsichere Näherungen in cm: $24 \sim \frac{1}{3}70$, $37 \sim \frac{1}{2}50$, $15 \sim \frac{1}{3}23$, $24 \sim \frac{1}{4}66$, $23 \sim \frac{1}{3}79$, $12{,}5 \sim \frac{1}{3}99$, $47 \sim \frac{1}{2}89$, $23 \sim \frac{1}{4}70$, $35 \sim \frac{1}{8}110$, $11 \sim \frac{1}{8}65$. Ein kleiner Fortschritt mit der Einübung ist nicht zu verkennen, wäre aber wahrscheinlich bei längeren

[3] Wilhelm Wundt, *Grundzüge der physiologischen Psychologie*, III, 1911, 469.
[4] Meumann, *op. cit.*, 314.

Versuchen Meumann's auch nicht ausgeblieben. Die Versuchsperson (Vp.) kommt eben ganz von selbst den natürlichen Hilfen zur zweckmässigsten Lösung der Aufgabe näher, die in dem Wesen der Raum- und Zeitvorstellung bereit liegen. Wir wollen sie im folgenden ausfindig machen und eine sinngemässe systematische Instruktion der Vp. zur bestmöglichen Lösung solcher Teilungsaufgaben nach dem "Augenmass" der Zeitanschauung daraus ableiten, die wir in unseren hier zu beschreibenden Versuchen von Anfang an verwirklicht haben.

Zur Lösung der Aufgabe, einen Bruchteil $(1/n)$ N einer gegebenen Normalgrösse N zu finden, gehört offenbar schon im Gebiet der anschaulichsten Gesichtswahrnehmung frontal gelegener Strecken sinngemäss *eine gedankliche Konstruktion der Teilstrecke* $(1/n)$ N *aus dem gegebenen* N, *bei der die unsere Aufgabe konstituiernden Zahlbegriffe klar und deutlich zu den gegebenen und hinzu gedachten Grössen in Beziehung gebracht werden müssen.* Es ist die Konstruktion, die Meumann selbst nennt, aber dem Denken seiner Vpn. ausdrücklich abspricht: *Projizieren der einzustellenden Strecke auf das einzuteilende Ganze in der blossen Vorstellung.* Auch bei der Beurteilung der Verhältnisse *frontaler* Strecken in der vorhin beschriebenen Gruppierung *a-b* und *c-d* muss man die Strecke *a-b* durch eine willkürliche Betätigung der produktiven Phantasie in n gleiche Teile geteilt denken oder *c-d* durch Hinzufügung von n—1 gleichen Strecken zu einer mit *a-b* vergleichbaren Dimension ergänzen. *Solche gedankliche Operationen sind aber auf dem Gebiet der Zeitvorstellung in analoger Weise durchaus möglich.* Man kann sich die Normalstrecke sogleich nach ihrem Anhören in der Phantasie in n gleiche Teile geteilt wiederholt denken und hat die Bruchteile vor sich, mit denen man einen isoliert darnach dargebotenen so genau vergleichen kann, als es bei einem isoliertenTaktelement neben dem in eine Reihe eingeordneten möglich ist. Das ist jeder musikalisch geübten Vp. ohne weiteres geläufig. Ebenso wie man aber bei einem Raumvergleich den gedachten Bruchteil bei Beherrschung einer entsprechenden Darstellungstechnik auch durch Nachzeichnen verwirklichen kann, so braucht man natürlich auch bei der zeitlichen Konstruktion in der Phantasie nicht auf die maschinelle Realisierung eines Vergleichs-Bruchteiles zu warten, sondern kann ihn sofort nach N selbst aus der Phantasie nachtaktieren. Auch Meumann schritt anscheinend zur Registrierung des nachtaktierten Bruchteiles auf dem mit dem Zeitsinn-Apparat verbundenen Kymographion, worüber er nach der zitierten Stelle weiter berichtet:

Ähnliche Versuche habe ich auch mit der Wiedergabe von Zeitunterschieden durch Taktieren gemacht. Sie zeigen keine grössere Genauigkeit, wenn man dabei die Ver-

suchsperson zwingt, die künstlichen Hilfen, wie Zählen, Rhythmisieren der Schläge u.s.w. zu gebrauchen. Es ist bemerkenswert, dass der Zwang, die Bewegungen rhythmisch zu gestalten, dabei fast unwiderstehlich ist. Gestattet man die Rhythmisierung der Bewegungen, so werden die Ergebnisse besser, bleiben aber auch jetzt noch von beträchtlicher Ungenauigkeit.[5]

Es ist freilich aus dieser Schilderung, die zuletzt auf eine leider nicht mehr erfolgte Veröffentlichung verweist, nicht sicher, ob hierbei auch wirklich die nämliche Aufgabe der Herstellung eines Bruchteiles einer gegebenen Normalzeit zu lösen versucht wurde. In diesem Falle würde aber natürlich der Versuch, ein rhythmisches Zählen von 1 bis *n* mit der Normalzeit zur Deckung zu bringen, jene "Projektion" der zur richtigen Teilung erforderlichen gedanklichen Konstruktion auf *N* sein. Ein Verbot *dieses* Zählens wäre bei unserer Aufgabe, zu der die Zahl *n* als konstituierender Bestandteil gehört, eine Art contradictio in adjecto gewesen. Freilich würde *bei Wiederholung der Normalzeit N* die Auswahl des passenden Tempos *sogar an der objektiven Zeitstrecke möglich* gewesen sein, und so ist es eigentlich verwunderlich, wenn die Teilung auch bei Zulassung des Zählens noch "mit beträchtlicher Ungenauigkeit" erfolgte. Immerhin sind gerade auch bei kurzen Zeiten, zumal wegen der Verschiedenheit des äusseren Nachtaktierens vom rein gedanklichen oder schwach artikulierten Mittaktieren, gewisse Schätzungsfehler im Spiele, an denen auch die noch so häufige Wiederholung bestimmter Zeitintervalle im alltäglichen Leben nicht viel ändert.[6] Für die Anschaulichkeit der Zeitvorstellung dürften aber diese Schätzungsfehler mit Bezug auf die *objektiven* Normalzeiten erst von sekundärer Bedeutung sein, so wenig wie die Beeinflussungen räumlicher Teilungsaufgaben durch optische Täuschungen die Anschaulichkeit der optischen Raumvorstellung leugnen lassen. *Zur Beurteilung der Möglichkeit einer unmittelbaren Zeitteilung müssen wir also die Teile nicht auf die Normalstrecke N, sondern auf eine ungeteilt hergestellte Vergleichsstrecke V beziehen.* Kurz, wir haben als Analogie zur Beurteilung der Raumteilung einfach *die Summe* aller *n* Taktelemente bei *geteilter* Vergleichsstrecke *V mit dem ungeteilten V zu vergleichen.*

Wenn wir aber die volle Leistungsfähigkeit der nach der Wahrnehmung einer Zeit *N* von ihr vorhandenen Zeitanschauung an der Lösung

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[5] *Ibid.*, 314.

[6] So fand Helmut Koehnlein in seinen Versuchen (Über das absolute Zeitgedächtniss, *Zsch. f. Sinnesphysiol.*, 65, 1934, 35-57, S. 50 ff.), die E. von Skramlik in seinem physiologischen Institut in Jena unter mehreren lehrreichen Arbeiten über den Zeitsinn anregte, dass bei der ganzen Sekunde Streuungen von ±6,2 bis ±23,7% vorkamen, während die langen Zeiträume, viel besser geschätzt wurden, z.B. die Minuten nicht selten mit ±0,1%. Auch absolute Schätzungen von Bruchteilen der Secunde (½ bis = ⅕) wurden mit ähnlichem Erfolg versucht.

einer solchen Teilungsaufgabe prüfen wollen, *dürfen wir freilich auch die Normalzeit N nicht wiederholen*, weil ja sonst *an ihr* jene Deckung der Zählung von 1 bis *n* allmählich ausprobiert werden kann. Wir wollen aber die Teilung nur auf die fertig vorschwebende Zeitvorstellung gründen, die sofort nach Abschluss der Norm *N* einen anticipierenden Ausblick auf die bevorstehende *Vergleichszeit* ermöglicht, in die bereits vom ersten Augenblick an die gewünschte Teilung gedanklich hineinkonstruiert oder "projiziert" werden kann. Natürlich sind hierin sogleich *alle n Teile* angelegt, wie bei einer Raumteilung, und die ausdrückliche Beschränkung auf die taktierende Verwirklichung *eines einzigen* würde nur neue Täuschungsmotive und Herstellungsfehler hineinbringen, da sich die Teilung impulsiv auszuwirken strebt und daher zu dieser Beschränkung besondere Hemmungsimpulse benötigt würden.

Versuchsanordnung und Instruction

Wie es bisher begründet wurde, haben wir in unserer Untersuchung, über die nun zu berichten ist, bei jedem einzelnen Versuch eine der Vp. nicht im voraus bekannte Normalzeit *N*, die wie bei Meumann mittels des Wundt' schen Zeitsinn-Apparates durch zwei Hammerschläge begrenzt wurde, *nur ein einziges mal dargeboten*, und dann hatte die Vp. sofort nach einer ihr völlig überlassenen Pause das Intervall *N* mit einem Reaktions-Taster in *n* möglichst gleiche Teile zerlegt oder ungeteilt, *wie verabredet, nachzutaktieren*. Um bei der Ablesung am Kymographion keinen Zeitfehler einzuführen, wurde das Deprez-Signal, das diese Taktierung aufschrieb, auch mit dem Hammer und seinem Ambos parallel geschaltet und jeder Hammerschlag mit der gleichen Latenzzeit wie der Taktschlag registriert. Eine "Willenszeit" kommt nicht als Reaktions-Zeitfehler, sondern nur als mittlere Variation in Betracht, da die Vergleichszeit-Grenzen synchronisierend, nicht abwartend, registriert wurden.

Da bei solchen Versuchen alles auf die möglichst konstante Durchführung einer bestimmten inneren Haltung (intern Behavior) ankam, so war ich wie bei meinen ersten Aufmerksamkeits-Versuchen selbst die Haupt-Vp. I. Die Zeitstrecken *N* wählte ich in *7 Stufen von ca. 1,5 bis ca. 3 sec.*, also wesentlich länger als Meumann. Denn fürs erste kam es nach der Feststellung, dass die Vergleichung nicht mit *N*, sondern mit der ungeteilten Vergleichsstrecke *V* entscheidend sei, überhaupt nicht mehr so sehr auf die Vermeidung grösserer Schätzungsfehler an, die bei grösseren *N* unvermeidlich sind, sondern viel mehr darauf, *dass die herzustellenden Teilstrecken* bei grösserer Teilzahl *n nicht zu klein ausfallen* und die Nachtaktierung erschweren. Da ich aber durch meine kürzlich veröffentlichten Versuche über rasche Rhythmisierungen ohnehin im schnellen regelmässigen Taktieren besonders geübt war,[7] so ging ich von $n = 2$ bis $n = 9$, unter Weglassung des leichteren $n = 8$. Als kürzeste Teilzeit bei $n = 9$ und $N = 1{,}5$ sec. war also ca. 0,16 sec., als längste bei $n = 2$ und $N = 3$ sec. ca. 1,5 sec. zu erwarten.

Als Experimentator fungierte bei meinen eigenen Taktierungen Herr Maschke, ein Laborant des psychophysischen Seminares, dem ich auch an dieser Stelle, zugleich für

[7] Wilhelm Wirth, Die Zeitverhältnisse bei schnellem rechtshändigen Taktieren mit und ohne Nebenaufgabe der Rhythmisierung, *Arch. f. d. ges. Psychol.*, 98, 1937, 245.

Unterstützung bei den Ausmessungen und Rechnungen, bestens danken möchte. Er wahrte völlige Unwissenstlichkeit durch Untermischung der *N* für jedes (gruppenweise konstant gehaltene) Teilungsschema. Dabei half ihm noch eine zufällige Variation bei jeder Einstellung des Apparates, die durch die Ausnützung jedes Umlaufes des Kymographions bei seinem Anlaufen in jedem Versuch in den Zeitwert hineinkam, und allerdings die Zeitbestimmung durch genaues Auszählen aller Stimmgabelschwingungen der stets mitschreibenden Kontrollgabel etwas erschwerte. Zur Kontrolle meiner Resultate liess ich aber die nämlichen Versuche auch von einer jungen südafrikanischen Hörerin des Seminares, Fräulein Wiblinger, ausführen, wobei ich selbst Versuchsleiter war und auch bei Konstanz der Umlaufsgeschwindigkeit (durch Auslassung des Anlaufes der Trommel) auf völlige Unwissenstlichkeit rechnen konnte, auch wenn ich nur die 5 mittleren Stufen des *N* meiner eigenen Serie benützte. Auch in den Einteilungsformen beschränkte ich mich auf die vier bis zur Fünfteilung. Bei dieser Vp. II hatte ich schon bei Seminar-Übungen in einer Reihe von 18 Einzelversuchen nach der Konstanzmethode eine grosse Sorgfalt des Urteilens festgestellt, und das halbe Idealgebiet der Gleichheitsfälle für $N = 1$ sec. mit einer einzigen "Verkehrtheit" der Urteilshäufigkeiten etwa gleich 1/25 gefunden. Sie hatte etwas musikalische Übung und fand sich sofort in die Aufgabe hinein. Auch ihr sei für ihre Mitarbeit gedankt.

Die Hauptsache war die Festsetzung einer ganz bestimmten Art *der gedanklichen Teilungs-Konstruktion*. Wir zählten zunächst *vor* der Darbietung von *N* einige Male von 1 bis n in einem bequemen Tempo, aber mit besonderer Betonung des ersten und letzten Schlages, und zwar auch bei der Aufgabe zur Herstellung der *ungeteilten* Strecke 1′, 2′. Schon hierbei wurde auf ungefähre Konstanz der Gesamtzeit für 1 bis *n* gesehen, also bei $n = 2$ oder 3 langsam, bei $n = 7$ oder 9 aber mit einer Art von Steno-Artikulation gezählt, die qualitativ noch klar differenziert und allgemeiner als "Kurz-Ausdruck" oder "Stenektypie" bezeichnet werden kann und bei allem Denken eine wichtige Rolle spielt. Nun kam die Darbietung der Normalzeit N, bei der sogleich der erste Schlag mit 1′, der letzte mit *n*′, also bei $n = 5$ z.B. mit 5, nachzubetonen war, während dazwischen, ohne Möglichkeit eines konkreten Durchzählens wegen der Unkenntnis von *N*, eine möglichst stetige Haltung jede Störung des nach Kenntnisnahme des *N* zu wählenden Rhythmus zu vermeiden suchte. Mit dem Abschluss von *N* konkretisierte sich dann sofort eine antizipierende Vorstellung, aus der heraus bereits die Pause so gewählt wurde, dass man in einem bestimmten Augenblicke mit einer wieder mit 1′ betonten Taktierung zur rhythmischen Ausführung der *n* Taktschläge so einsetzte, dass dies von der Vorstellung des gehörten Intervalles als eines ersten "Taktes" dieses Rhythmus beherrscht wurde und mit allen *n* Schlägen das nämliche Ganze, also den gehörten ganzen "Takt" nach taktierte.

Diese proportionale "Projektion" eines schon vorher in beliebigem Tempo vorschwebenden Zähl-Schemas auf die durch das Anhören von *N* ins Bewusstsein getretene Zeitvorstellung steht also in naher Beziehung zu der noch allgemeineren Möglichkeit eines "*Proportionsvergleiches von Zeitintervallen*,"[8] über die Bühler in seinem wertvollen

[8] Vgl. Wirth, *Psychophysik*, 1912, 419. Die dort noch genannte Abhandlung von Edith A. Alvord and Helen E. Searle (A Study in the comparison of time intervals, this JOURNAL, 18, 1907, 177-182) war von der unserigen bei äusserer Ähnlichkeit der Fragestellung doch principiell verschieden, da nach Darbietung einer Normalzeit von 3 bis 10 sec. ein 10 bis 12 mal *grösseres* Intervall als Vergleichszeit *V* dargeboten und *gefragt wurde, wie oft N in V enthalten ist*. Man konnte also hierbei naturgemäss sogleich das *N* selbst als rhythmische Einheit festhalten und auf *V* sozusagen abtragen. In den sorgfältig beschriebenen Selbstbeobachtungen spielten *Nebenvorstellungen* von Bewegungen eine wichtige Rolle, *während bei unserer Aufgabe zu solchen Reflexionen gar keine Zeit war. Denn die Vp. musste ja unmittelbar*

Buche, "*Die Gestaltwahrnehmung*", berichtet.[9] Doch blieb auch bei ihm wie bei allen früheren Versuchen über das Optimum der Unterschiedsschwelle ein bestimmtes Verhältnis als Normal-Proportion in einer ganzen Versuchsreihe konstant. Auch waren es stets nur *zwei* Teile der gegliederten Gesamtstrecke, die ausserdem, dem Zweck der Versuche entsprechend, mit einer passiv gehörten Proportion zu vergleichen war und bei den ersten Versuchen, in denen die Unterschiedsschwelle für die Wiedererkennung der nämlichen Proportion in einem anderen Tempo auf 1/103 herab getrieben werden konnte, wesentlich kürzer war als unsere Zeiten. In späteren Versuchen mit etwas längeren Zeiten fand Bühler für die Proportion 0,7 sec: 1,12 = 0,5 sec.: x die Genauigkeit der Bestimmung des x als 1/26.[10]

An und für sich lässt sich natürlich das Problem auch auf reine *Intensitätsverhältnisse* übertragen.[11] Damit aber diese zu ähnlich unmittelbarer Teilung von Quasi-Strecken der übermerklichen Intensitäts-Unterschiede befähigen, müssten sich eben, durch Nebeneinander-Ordnung der zu vergleichenden Stufen der Intensität, im Bewusstsein erst Quasi-Extensionen ausbilden, wie sie in den extensiven Vorstellungen von Raum und Zeit bereits durch die natürliche seelische Entwickelung bereit liegen. Was sie für die rasche Abteilung von Strecken leisten, kann aber jeweils nur durch das Experiment gefunden werden.

Die Versuchsergebnisse

(*a*) *Die Tabellen und Kurven.* Bei Vp. I entfielen 24 Versuche auf die *ungeteilte* Herstellung der Vergleichszeit V zu den 7 Normalzeiten N, bei Vp. II 10 Versuche bei nur 5 N. Auch die Halbierung ($n = 2$) wurde bei beiden Vpn., als erste Form der Teilung überhaupt, an späterer Stelle wiederholt, mit insgesamt 13 Versuchen bei Vp. I und 9 bei Vp. II. Doch zeigte sich bei der zweiten Reihe mit Vp. II wegen des Endes der Stunde Ermüdung, so dass sie nach nur 4 N abgebrochen wurde. Die Ergebnisse dieser Reihe sind nicht in die Tabellen einbezogen, aber berechnet und unten diskutiert. Sonst fanden nur noch 3 Wiederholungen der *Dreiteilung* mit Vp. II statt. Bei allen anderen Reihen kam jedes N mit jeder der Teilerzahlen n nur einmal vor. Die Gesamtzahl der Versuche für die sieben N und acht n der Vp. I betrug daher 83, für

nach N schon zu dem richtigen Rhythmus bereit sein. Bei der langen Zeit N war der Rhythmus bei ihrer Abtragung auf V kaum ein sehr kräftig markierter.

Eine *neue* Aufgabe käme erst in Frage, wenn ein Verhältnis $N : V$ zu beurteilen wäre, bei dem N nicht in V aufgeht, sondern nach einem gemeinschaftlichen Teiler gesucht werden muss, wie etwa beim Verhältnis 5 : 7, wo die in beiden aufgehende Einheit zu suchen ist. Doch führte dies naturgemäss zur Projektion einer adäquaten rhythmischen Einheit auf *zwei* von *jedem* der beiden Zeiten stammende Zeitvorstellungen V^1 und V^2, oder sogleich auf N^1 und N^2, eine Leistung, die man bei der Untersuchung des unmittelbaren Zeitvergleiches gerade auszuschalten suchte. Ein besonderes Interesse vom Standpunkt der Betrachtung des Proportions-Vergleiches hätte aber eine solche gedankliche Konstruktion kaum.

[9] Karl Bühler, *op. cit.*, 1913, 223.

[10] *Op. cit.*, 272.

[11] Vgl. E. W. Scripture, Researches on voluntary effort: Influence of the rate of change upon the perception of differences in pressure and weight, *Stud. Yale Psychol. Lab.*, 4, 1896, 69-75.

die fünf *N* und fünf *n* der Vp. II 34. Sie wurden für Vp. I in drei und für Vp. II in einer Sitzung erledigt. Eine Steigerung der Leistung durch längere Einübung war ja nicht erstrebt, sondern es sollte die bereits vorhandene Fähigkeit zu einer solchen unmittelbaren Teilung einer gegebenen Zeitstrecke bei Personen geprüft werden, die mit der Herstellung von ungeteilten Vergleichszeiten durch Taktieren bereits vertraut sind.

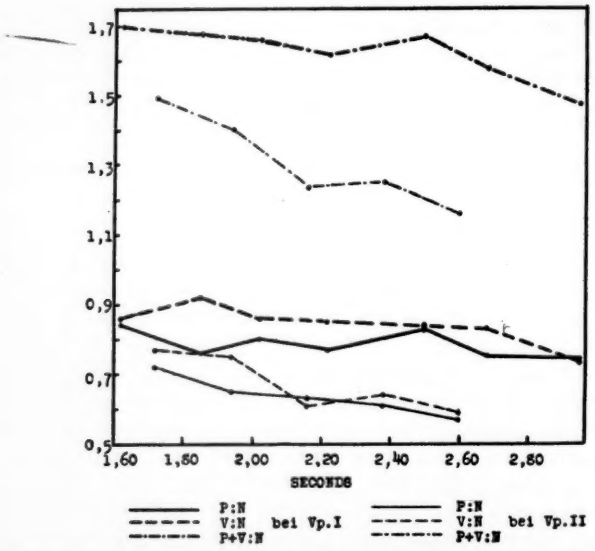

FIG. 1. DIE ABHÄNGIGKEIT DER MITTLEREN VERHÄLTNISSE DER PAUSEN *P* UND DER VERGLEICHSSTRECKEN *V* VOM NORMAL-INTERVALL *N*
(Vgl. Tabelle II and IIa)

Wir wollen nun zunächst die Anlage aller folgenden Tabellen und Kurven beschreiben, da sie bei der Erläuterung der Resultate bereits auf einander zu beziehen sind. Nachdem alle Kurvenstrecken in Zeitwerten auf 0,01 sec. genau ausgedrückt waren, wurde für jeden Einzelversuch das Verhältnis *V* : *N* der geteilt bezw. ungeteilt hergestellten Vergleichszeit *V* zur Normalzeit *N* bestimmt, in der das entscheidende Hauptergebnis enthalten ist, sowie das für die Arbeitsweise der Vp. ebenso charakteristische Verhältnis *P* : *N* der zwischen *N* und *V* gelegenen Pause *P*, endlich die Zeiten der sämtlichen n Teile jeder Vergleichsstrecke und deren mittlere Variation innerhalb der ganzen

Strecke *V*. Doch enthalten die Tabellen, ausser der konkreten Wiedergabe aller *n* Teile für je ein Beispiel der 8 Teilungsformen der Vp. I in Tabelle V und für die 5 Teilungsformen der Vp. II in Tabelle Va, *ausschliesslich arithmetische Mittel* jener Verhältnisse $P : N$ und $V : N$ mit ihren mittleren Variationen in Tabelle II, IIa, III und IIIa, ebenso die arithmetischen Mittel der mittleren Variationen der *n* Teile innerhalb einer Vergleichsstrecke *V* in Tabelle IV and IVa. In Tabelle II

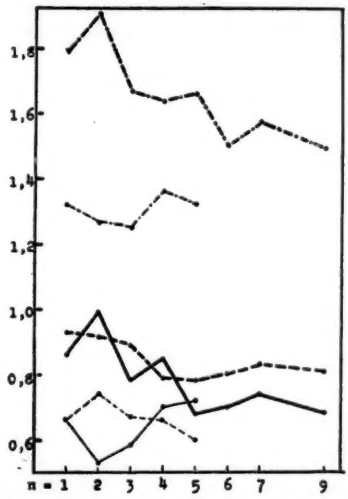

FIG. 2. DIE ABHÄNGIGKEIT DER MITTLEREN VERHÄLTNISSE DER PAUSEN P UND DER VERGLEICHSSTRECKEN V VON DEM NENNER *n* DER TEILUNG.
(Die Bedeutung der Linien ist die nämliche wie in Fig. 1; Vgl. Tabelle III und IIIa.)

und IIa sind jene Verhaltnisse in ihre mittleren *Abhängigkeit von der Länge der zu teilenden Strecke* dargestellt, also nach den Normalzeiten *N* geordnet. Dabei ist die ungeteilte Herstellung in das Gesamtmittel der Verhaltnisse der V^{1-9}, bezw. V^{1-5} mit eingerechnet, ausserdem aber in der vorletzten Zeile gesondert daneben gestellt, damit sie mit den früheren Resultaten dieser üblichen Herstellung im ganzen verglichen werden kann. In Tabellen III und IIIa ist dagegen die mittlere *Abhängigkeit jener Verhaltnisse von der Anzahl der Teile n* zu erkennen, in die *V* zu zerlegen war. Dort bilden also alle Versuche mit dem nämlichen *N* bei Variation des *n*, hier alle mit dem nämlichen *n* bei Variation des *N* einen Kollektivgegenstand, der durch die geringe mittlere Variation die Mittelbildungen als berechtigte erscheinen lässt. In diesen vier

Tabellen ist schliesslich überall in der letzten Zeile noch der relative Wert *der Summe von P und V* als eine Art Mittelbildung aus den psychologisch verwandten Grössen *P* und *V* angegeben. Sämtliche arithmetischen Mittel der Tabellen II und IIa sind ausserdem in der Fig. 1, und die Mittel aus Tabelle III und IIIa in Fig. 2 dargestellt, in der die genannten Abhängigkeiten mit einem Blick zu erfassen sind.

TABELLE I

DIE MITTELWERTE UND STREUUNGEN DER 7 GRUPPEN DER NORMALZEITEN *N*
(Vp. I)

Gruppe	1	2	3	4	5	6	7
N	1,62	1,85	2,03	2,22	2,50	2,68	2,95
M. Var.	0,06	0,08	0,03	0,05	0,05	0,06	0,06
Extreme	1,52-1,70	1,77-2,04	1,94-2,08	2,15-2,28	2,39-2,62	2,60-2,81	2,88-3,19

TABELLE II

MITTLERE VERHÄLTNISSE *P/N* UND *V/N* DER IN *n* TEILEN HERGESTELLTEN VERGLEICHSZEITEN *Vn* FÜR DIE 7 GRUPPEN DER NORMALZEITEN *N* NACH TABELLE I
(Vp. I; vgl. Fig. 1)

	Gruppe 1	2	3	4
P : *N*	0,86	0,92	0,86	0,85
M. Var.	0,10	0,05	0,08	0,10
V^{1-9}: *N*	0,84	0,76	0,80	0,77
M. Var.	0,07	0,09	0,09	0,06
V^1 : *N*	0,98(0,13)	0,82(0,07)	0,80(0,07)	0,77(0,13)
(*P*+*V*):*N*	1,70	1,68	1,66	1,62

	5	6	7	Arith. Mittel 1-7	Mittel 2-6
P : *N*	0,84	0,83	0,73	0,841	0,860
M. Var.	0,05	0,06	0,07	0,073	0,068
V^{1-9}: *N*	0,83	0,75	0,74	0,784	0,782
M. Var.	0,10	0,09	0,15	0,093	0,086
V^1 : *N*	0,98(0,04)	0,77(0,14)	0,85(0,04)	0,854	0,828
(*P*+*V*):*N*	1,67	1,58	1,47	1,611	1,642

TABELLE IIa

MITTLERE VERHÄLTNISSE *P/N* UND *V/N* FÜR DIE 5 NORMALZEITEN *N*
(Vp. II; vgl. Fig. 1)

N	1,72	1,94	2,16	2,38	2,60	Arith. Mittel
P : *N*	0,77	0,75	0,61	0,64	0,59	0,670
M. Var.	0,05	0,07	0,04	0,07	0,05	0,060
V^{1-5}: *N*	0,72	0,65	0,63	0,61	0,57	0,638
M. Var.	0,09	0,07	0,06	0,06	0,09	0,07
V^1 : *N*	0,78(0,01)	0,65(0,01)	0,61(0,02)	0,65(0,00)	0,60(0,08)	0,658(0,02)
(*P*+*V*):*N*	1,49	1,40	1,24	1,25	1,16	1,308

TABELLE III

ABHÄNGIGKEIT DER VERHÄLTNISSE P/N UND V/N VON DER ANZAHL n DER TEILE DER VERGLEICHSZEIT V

(Vp. I; vgl. Fig. 2)

Teile	1	2	3	4	5	6	7	9
$P : N$	0,93(0,01)	0,92(0,02)	0,89	0,79	0,78	0,80	0,83	0,81
M. Var.	0,07(0,01)	0,09(0,01)	0,06	0,12	0,08	0,05	0,04	0,07
$V : N$	0,85(0,01)*	0,99(0,07)	0,78	0,85	0,68	0,70	0,74	0,68
M. Var.	0,09(0,05)	0,08(0)	0,06	0,05	0,05	0,05	0,07	0,06
$(P+V):N$	1,78	1,91	1,67	1,64	1,46	1,50	1,57	1,49

* Diese mittlere Variation besteht zwischen den drei Reihenmitteln bei ungeteilem V, ist also nicht das (0,13 betragende) Mittel der Variationen der zum nämlichen N gehörigen V aus Tabelle II, vorletzte Zeile.

TABELLE IIIa

ABHÄNGIGKEIT DER MITTLEREN VERHÄLTNISSE $P:N$ UND $V:N$ VON DER ANZAHL n DER TEILE DER VERGLEICHSZEIT

(Vp. II)

Teile	1	2	3	4	5
$P : N$	0,66	0,74	0,67	0,66	0,60
M. Var.	0,06	0,08	0,07	0,07	0,08
$V : N$	0,66	0,53	0,58	0,70	0,72
M. Var.	0,05	0,03	0,02	0,05	0,04
$(P+V):N$	1,32	1,27	1,25	1,36	1,32

TABELLE IV

PROZENT-VERHÄLTNIS DER MITTLEREN VARIATIONEN DER n TEILE ZUM MITTEL $(1/N)V$

(Vp. F)

Mittleres N	$n=2$	$n=3$	$n=4$	$n=5$	$n=6$	$n=7$	$n=9$	Arith. Mittel.
1,62	8(3)	5	6	7	6	(9)	4	6,0
1,85	5	9	4	3	8	7	(6)	6,0
2,03	5(3)	6(2)	–	9	7	5	6	6,0
2,22	0,5(0,5)	8	2	8	8	–	8	5,8
2,50	3(2)	2(0,7)	6	6	7	6(0,5)	5	5,0
2,68	8(5)	4(1)	4(1)	9	7	6	6	6,4
2,95	6(4)	4	3	10	7	6	9	6,4
Arith. Mittel.	5,2(2,5)	4,7(0,7)	4,3	7,3	7,1	6,0	6,6	5,9% 2-5
M. Var.	2,3(1,1)	2,2(0,3)	1,3	1,8	0,6	0,5	1,5	5,4%

TABELLE IVa

PROZENT-VERHÄLTNIS DER MITTLEREN VARIATIONEN DER n TEILE ZUM MITTEL $(1/N)V$

(Vp. II)

N	$n=2$	$n=3$	$n=4$	$n=5$	Arith. Mittel
1,72	2	4	5	3	3,5
1,94	2	2	5	5	3,5
2,16	4	5	3	6	4,5
2,38	2	6	3	3	3,6
2,60	4	6	4	4	4,2
Arith. Mittel	2,8	4,6	3,9	4,1	3,6%
M. Var.	0,9	1,2	0,6	1,3	

TABELLE V

DIE *n* TEILE DER GETEILTEN VERGLEICHSSTRECKEN *V* FÜR *N* 2,50(2,39-2,62) SEC.

(Vp. I)

			Nummer des Teiles								
N	*V*	$(1/n)V$	1	2	3	4	5	6	7	8	9
2,40	2,66	1,33	1,09	1,00							
2,47	2,11	0,703	0,70	0,69	0,72						
2,62	2,36	0,590	0,62	0,56	0,63	0,55					
2,50	1,47	0,294	0,30	0,31	0,27	0,28	0,31				
2,62	1,97	0,328	0,38	0,31	0,31	0,30	0,34	0,35			
2,40	1,98	0,283	0,33	0,30	0,29	0,29	0,29	0,24	0,27		
2,39	1,92	0,213	0,24	0,22	0,20	0,20	0,22	0,21	0,20	0,20	0,22

TABELLE Va

DIE *n* TEILE DER GETEILTEN VERGLEICHSSTRECKE FÜR *N* 2,38

(Vp. II)

			Nummer des Teiles				
N	*V*	$(1/n)V$	1	2	3	4	5
2	1,23	0,615	0,60	0,63			
3	1,37	0,442	0,50	0,42	0,46		
4	1,67	0,417	0,41	0,40	0,41	0,44	
5	1,50	0,300	0,29	0,31	0,30	0,30	0,30

Die Normalzeiten wurden vom Experimentator als Kontakt-Abstand 70°, 80°, 90°, 100°, 110° und 120° vom Anfangskontakt bei 0° eingestellt. Da aber bei Vp. I die Zeit für jede Kontakt-Distanz zufällig etwas variierte, so sind in Tabelle I die sieben *Mittelwerte N* für die sieben Einstellungen mit ihren mittleren Variationen und Extremen vorausgeschickt, wobei 5 Versuche mit besonders stark abweichendem Zeitwert *N* in eine andere Gruppe übergeführt wurden. Für Vp. II mit der konstanten Zeit für jede Einstellung des *N* kam natürlich eine Parallele zu Tabelle I in Wegfall.

Die Versuche wurden in Gruppen zu je 7 bezw. 5 bei Vp. I bezw. II mit der nämlichen Teilungsaufgabe, also konstantem *n*, durchgeführt. Wo für eine Mittelbildung zur Ableitung der Tabellen mehr ganze Reihen zur Verfügung standen, sind Mittel aus deren Mitteln gebildet und mit einer in Parenthese beigefügten mittleren Variation in den Tabellen als solche besonders gekennzeichnet worden, da aus dieser Variation auch auf die Konstanz bei Wiederholung des nämlichen *N* geschlossen werden kann. Bei Tabelle IV ist eine solche Wiederholung auch für jede Einzel-Konstellation von *n* und *N* angegeben, da ja hier jede solche selbständig zur Geltung kommt. Den Lücken bei $n = 4$ und 5 infolge der Verschiebung des Versuches in eine andere *N*-Gruppe entspricht eine Vermehrung der Versuchszahl bei dieser anderen Gruppe, so dass dort eine Mittelbildung eintritt. Bei $n = 4$ ist diese übrigens gleich um drei Stellen von der Lücke entfernt, weil hier noch eine weitere, diese Anhäufung in der nächsten Nachbarschaft wieder aufhebende Verschiebung dazwischen lag.[12]

[12] Cf., *supra* 83 f.

(*b*) *Die Gesetzmässigkeiten.* Der wichtigste Anhaltspunkt für die Beurteilung, ob die eingeteilte Vergleichszeit *V* wirklich unmittelbar aus einer Vergegenwärtigung des Normal-Intervalles *N* heraus hergestellt wurde, und nicht einfach in einem beliebigen adäquaten Zählrhythmus, sollte das *ungeteilte V* für das nämliche *N* bilden. Deshalb wollen wir dieses zunächst in Tabelle II (bezw. IIa) in der vorletzten Zeile ins Auge fassen. Dabei fällt nun die starke mittlere Verkleinerung des *V* auf 0,854 (bezw. sogar 0,658) auf, während *V* in den Versuchen von Glass[13] nach der Herstellungsmethode in unserem Zeitbereich dem *N* viel näher bleibt und bei Kahnt[14] der ebenfalls negative Schätzungsfehler nur ca 1/16 von *N* erreicht. Allerdings folgt bei Glass *V* ohne Pause auf *N*, und bei Kahnt ist zwar eine ebenfalls willkürlich hergestellte Pause eingeschoben, aber das folgende *V* ist bei ihm nicht hergestellt, sondern passiv wahrgenommen und nach der Konstanz-Methode beurteilt. Andererseits ist bei unseren Versuchen, wenigstens bei Vp. I., die mittlere Variation bei der Wiederholung der Versuche für die einzelnen *N* etwa 0,09, und das obere Extrem der Mittel reicht bei dem ersten und fünften *N* sogar bis 0,98, also fast bis 1,0 hinauf. Diese Variation beim Wechsel des *N* ist aber ja grossenteils überhaupt nicht rein zufällig, sondern bedeutet die seit Glass bekannte *periodische Änderung.* Das zweite relative Maximum liegt auch bei Vp. II an der nämlichen Stelle wie bei Vp. I. Da man die Ursache der Unterschätzung von *N* in der assimilierenden Einmischung der Zeiteinheit des "adäquaten" Tempos der natürlichen Apperceptions-Schwankungen in der Nähe der Sekunde in die Erinnerung an *N* sehen kann,[15] so scheint es, als ob diese Einmischung bei aktiver Herstellung des *V* nach einer rhythmisch beeinflussten Pause sich besonders stark auswirkt, und zwar umso mehr, je mehr die Vp. durch die Herstellungsarbeit in Anspruch genommen ist, wie es bei Vp. II wohl der Fall war.

Vergleichen wir nun die in Teile *geteilten* Vergleichszeiten *V* mit diesen ungeteilten *V*, so zeigen die zu den nämlichen *N* gehörigen Mittel der Vp.I in der 3.,4. und 6. Gruppe zwei gleiche und einen fast gleichen Wert, und im übrigen die Höchstwerte an der nämlichen Stelle der 1. und 5. Gruppe., also genau die nämliche "Periode." Nur ist *V:N* überall noch etwas kleiner geworden, im Mittel um 0.07. Das nämliche gilt für Vp.II, bei welcher aber die Übereinstimmung noch

[13] Richard Glass, Kritisches und Experimentelles über den Zeitsinn, *Philos. Stud.*, 4, 1887, 423-456.
[14] O. Kahnt, Über den Gang des Schätzungsfehlers bei der Vergleichung von Zeitstrecken *Psychol. Stud.*, 9, 1914, 279-320.
[15] Vgl. Wirth, *Die experimentelle Analyse der Bewusstseinsphänomene*, 1908, 276 f.

grösser ist und die Differenz im Mittel nur 0,02 beträgt. Offenbar hat bei ihr eine Tendenz, die sich bei Vp.I erst mit der schwierigeren Leistung der Teilung geltend machte, schon bei der ungeteilten Herstellung so stark mitgewirkt, dass jetzt *V* gar nicht mehr viel darüber hinaus verkleinert werden kann. Übrigens ist auch bei Vp.I die mittlere Verkleinerung gegenüber der ungeteilten Strecke *V* etwas geringer, wenn man sich auf die bei Vp.II allein vorkommenden *N* der Gruppe 2–6 beschränkt, wie dies in der letzten Spalte der Tabelle II geschieht, wo die Differenz nur 0,046 beträgt.

Hiermit wäre also bereits *unsere Hauptfrage positiv beantwortet*, ob die gute Herstellung des Wertes $(1/n)$ *V*, die aus den geringen Fehlerprozenten der Tabellen IV bezw. IVa zu ersehen ist, wirklich *als eine unmittelbare Teilung des subjektiven Äquivalenz-Wertes von N betrachtet* werden darf. Dieser Eindruck wird aber noch wesentlich verstärkt, wenn man in der Zeile der Verhältnisse *P:N* der Tabellen II und IIa findet, dass *auch schon die Pause fast genau den nämlichen Mittelwert* zeigt wie die folgende Vergleichszeit *V*. Mit dem Ende von *N* holt also die Vp. sogleich in einem "Takte," dessen Tempo ihr zu *N* äquivalent erscheint, zu den beiden folgenden Taktschlägen am Ende von *P* und von *V* aus, gleichgültig ob in diesen Takt beim zweiten Male gleichzeitig eine Teilung eingefügt werden soll oder nicht.

Nun muss aber erst noch der mögliche Einwand abgeschnitten werden, dass jene grosse Annäherung der *Mittelwerte* aller geteilten *V* an denjenigen des ungeteilten kein eindeutiger Hinweis darauf zu sein brauche, dass jedem Normalintervall *N* ein bestimmtes Anschauungs-Äquivalent als Teilungs-objekt der anticipierenden Phantasie entspreche, sondern ein rein äusserlicher Effekt der Statistik sei. Es liesse sich vermuten, dass die wiederholte Beschäftigung mit einem gewissen Gesamtbereich aller *N und n* eine mittlere "Einstellung" erzeugt habe, die das *einzelne V* höchstens bei dem *mittleren N* und *n* trotz der Teilung richtig dem ungeteilten *V* gleich werden lasse, während im übrigen bei den Teilungsaufgaben keine entsprechende Nachbildung des jeweiligen *N* vorliege, sondern einfach in jenem *mittleren* Takte *n* Taktelemente hergestellt würden. Offenbar müsste aber dann das geteilte *V* einfach zu *n* proportional sein! Der Gegensatz der tatsächlichen Verhältnisse zu dieser fingierten Einstellung zeigt am besten, dass der Teilung stets ein zur adäquaten Vorstellung gut proportionales Anschauungs-Äquivalent zu grunde liegt. Abgesehen davon, dass die mittlere Variation der Mittelwerte für die einzelnen *N* in Tabelle II für jene groteske Konsequenz einer Proportionalität des *V* zu *n* viel zu

klein wäre, findet man in der Tabelle III, die für Vp.I die Abhängigkeit der Pause und Vergleichszeit von der Zahl *n* darstellt, *dass V mit der Zunahme von n, wenigstens bei dieser Vp., umgekehrt sogar abnimmt*, wie auch in Fig. 2 wieder mit einem Blick zu überschauen ist. Bei den vier kleinsten Teilungs-Nennern $n = 2,3$ und 4 ergibt sich sogar im Mittel 0,87, also fast genau der volle Wert des ungeteilten *V*. *Eine solche geringe Anzahl von Teilen ist also offenbar in der Antizipation noch ähnlich klar zu überschauen wie das ungeteilte Äquivalent von N.* Von $n = 5$ an aber sinkt das Verhältnis auf eine tiefere Stufe herab, die derjenigen des ungeteilten *V* bei der Vp.II nahe kommt, und bleibt ohne grössere Veränderung mit der Zunahme des *n* bis 9 auf dieser Stufe. Dabei geht die Pause dieser Abstufung wieder einigermassen parallel, so dass also der ganze Rhythmus (also auch $P + V$) durch die schnellere Zählung, die mit Vergrösserung von *n* notwendig wird, acceleriert erscheint, aber zugleich mit accelerato bei dem *V* mit höherem *N*.

Auch bei der Vp.II zeigt wenigstens die Pause nach Tabelle IIIa eine solche Abnahme mit dem Teilungs-Nenner *n*. Dagegen ist hier bei *V* in der Tat eine *Zunahme* mit *n* sehr deutlich; aber freilich nicht proportional zu *n*, sondern nur von etwa 5 : 7 beim Fortschritt des *n* von 2 : 5. Dass aber die Vp. sich nicht etwa ohne Rücksicht auf die Anpassung des *ganzen V* an *N* einfach durch die grössere Zahl von Teilen auf ein grösseres *V* bei den höheren *n* hinauftreiben liess, ergibt sich schon daraus, dass sie auch bei diesen *n* zu Protokoll gab, mit dem Schluss von *V zu früh* daran zu sein, so dass sie den letzten Teil *noch etwas länger* zu machen suchte. Ihre besonders starke Reduktion der Vergleichszeit verlief also unter dem Eindruck, durch etwas von der zuletzt wieder besser vorchwebenden Vorstellung des *N abgelenkt* worden zu sein. Solche Ablenkungen führen aber stets zu einer erhöhten Auswirkung natürlicher Fehler-Tendenzen, wenn nicht die ablenkende Vorstellung dem speciell entgegenwirkt. Diese natürliche Fehlertendenz geht aber oberhalb der Indifferenz-Zeit stets in Richtung der Verkleinerung des *V*. *Die besondere Ursache der stärkeren Ablenkung* im Vergleich zu Vp I erkennen wir beim Vergleich der Tabelle IVa dieser Vp. mit Tabelle IV der Vp.I. Das Gesamtmittel der mittleren Variationen der einzelnen Teile in Prozenten des *n*-ten Teiles von *V* beträgt nämlich bei Vp.II nur 3,6% gegen 5,4% bei den nämlichen Teilzahlen *n* der Vp.I. *Die Vp.II hat also auf die Herstellung einer genauen Gleichheit der Teile viel mehr Mühe verwendet als ich selbst.* Ich betrachtete ja die Gleichheit der Teile mehr als Nebenaufgabe im Vergleich zu der Hauptaufgabe einer möglichst richtigen Vergegenwärtigung von *N*,

deren Lösung zur Sicherung der Grösse des Bruchteiles $(1/n)$ V mehr Aufmerksamkeit erforderte als die genaue Gleichheit aller Teile. In deren regelmässiger Herstellung glaubte ich auch durch jene nicht lange vorher angestellten Versuche mit schnellem Taktieren in bestimmten Rhythmen[16] schon genügend vorgeübt zu sein, um die erforderliche Annäherung an den gewünschten Bruchteil zu erreichen, wenn nur das bei der Teilung vorschwebende *Ganze* richtig genug ausfiel. Natürlich hätte ich auch die Vp.II durch eine besondere Instruktion auf diesen Punkt noch besonders hinweisen können. Aber seine volle Bedeutung ist mir ja erst durch diese Ergebnisse bekannt geworden.

Dass diese Deutung der Zunahme von V mit n bei Vp. II, bezw. seiner Abnahme mit der Zunahme der Länge des einzelnen Teiles, von der die Schwierigkeit der genauen Teilung nach dem Weberschen Gesetz abhängt, das Richtige treffen dürfte, zeigte auch jene bereits erwähnte Reihe mit der Halbierung der Strecke, auf deren Einrechnung in die Mittelwerte ich verzichtete. Hier sank nämlich bei Vp.II das mittlere Verhältnis von $V : N$ sogar auf 0,45, bei einem unteren Extrem von 0,39, also kaum 40% der Normalzeit N. *Dafür stieg die Genauigkeit der Teilung so an*, dass z.B. für $N = 2{,}38$, auf das sich auch die Beispiele der Tabellen V und Va beziehen, die beiden Häfften genau gleich ausfielen und die mittlere Variation der Teile im Mittel der ganzen Reihe auf 1% sank. Es ist aber bekannt, dass gerade die Ermüdung, um derenwillen wir die Reihe mit ihrer starken Vernachlässigung der Hauptaufgabe einer möglichst richtigen *Dimension* des Bruchteiles oben in Wegfall kommen liessen, die Regelmässigkeit des Taktierens unter Umständen besonders begünstigen kann. *Wie viel* Aufmerksamkeit auf die einzelnen Seiten einer so komplizierten Aufgabe zu verwenden ist, um ein Optimum der Lösung zu erzielen, ist natürlich Sache einer besonderen Selbstkontrolle an der Hand des eigentlichen Sinnes der Hauptaufgabe, die im allgemeinen erst auf grund einer längeren generellen und besonderen Einübung möglich ist. In zwei Versuchen mit den grössten Anzahlen n bin ich (Vp.I) offenbar in der Unterordnung der Einteilung unter die Herstellung der Bruchteile aus dem richtigen Ganzen zu weit gegangen, da ich bei dem schnellsten Taktieren nach den kleinsten N einmal statt 7 nur 6, und einmal statt 9 nur 8 Teile herstellte (in Tabelle IV eingeklammert). Dies ist das (schon von mir in der genannten Arbeit über schnelles Taktieren)[17] erwähnte Unterdrückungs- oder Verschmelzungs-Phänomen, das dem Ranschburg'schen

[16] Vgl. Wirth, *op. cit.*, *Arch. f. d. ges. Psychol.*, 98, 1937, 245.
[17] Wirth, *ibid.*, 254.

verwandt ist. Es ist bei kleineren Zahlen nirgends vorgekommen und wäre wohl auch bei $n = 8$ nicht aufgetreten, auf das ich bei der grossen Einfachheit seiner Taktierung verzichtete.

Nach Tabelle IV habe ich das N mit dem geringsten, aus der letzten Kolumne ersichtlichen Mittelwert 5,0% der Variationen bei $N = 2{,}5$ ausgesucht, um *die zu grunde liegenden Teilungen in Tabelle 5 konkret zu veranschaulichen.*[18] Da die nächstliegende Stufe des $N = 2{,}38$ bei Vp.II ebenfalls einen niedrigen Wert in Tabelle IVa zeigt, so ist auch Tabelle Va für Vp.II als Analogon hierzu entwickelt. Man sieht nun zunächst, dass im einzelnen Falle in der Tat "*Verkehrtheiten*" von der Art der von Meumann genannten vorkommen können, indem ein Bruchteil mit grösserem Nenner n gelegentlich wirklich grösser ausfallen kann als ein Bruchteil mit kleinerem n. Es kommt eben dabei ganz darauf an, wie *das Ganze* im Vergleich zu N ausfällt. Wenn nun auch bei kleinerem n in unserem Beispiel für Vp.I, die weniger *genau* teilt als Vp.II, keine so grossen Fehler im *ganzen* V vorkommen, dass eine einigermassen gute Teilung zu einer solchen "Verkehrtheit" führen musste, so ist doch bei $n = 5$ in Tabelle V eine besonders starke Verkleinerung von V auf nur 59% des N, eine der stärksten bei Vp.I überhaupt vorgekommenen, eingetreten. Und so ist in der Tat $1/5\ N$ kleiner ausgefallen als die Mehrzahl der von dem $0{,}75\ N$ grossen V stammenden Sechstel der nächsten Zeile. Sonst kommt aber in der Tabelle nur noch eine einzige "Verkehrtheit" des geringeren Grades vor, dass überhaupt ein Teil bei höherem n grösser ausfällt als irgend ein Teil bei kleinerem n, wenn er auch nicht grösser wird als der richtige mittlere Bruchteil des V mit kleinerem n. Dieser einzige Fall ist das erste $1/7\ V$, das grösser ist als einige allerdings zu klein genommene Teile bei $n = 6$ (0,33 gegen 0,31 und 0,33). Für den bei Meumanns Versuchen allein in Betracht kommenden ersten Teil ist aber ausser jenem zuerst genannten Fall keine weitere Umkehrung der normalen Grössenordnung vorgekommen. Der erste Teil wird infolge einer Eigentümlichkeit der Rhythmisierung zumal bei Vp.I gern grösser genommen. Dass *bei Vp.II* trotz ihrer Tendenz der Zunahme des V mit n *keine Verkehrtheit* in Tabelle Va zu verzeichnen ist, verdient besonders hervorgehoben zu werden. Es zeigt, dass auch hier die Berücksichtigung des Ganzen anschaulich genug war, um die von Meumann verzeichneten schweren Fehler seiner Versuchspersonen zu vermeiden. Hierzu hat freilich auch wiederum die grössere Regelmässigkeit der Teile beigetragen, die auch bei gröss-

[18] Soweit Wiederholungen stattfanden, sind doch die ersten Versuche dargestellt.

eren Zunahmen des V mit n den Bruchteil nicht in den Bereich von $1/(n+1)\ V$ gelangen liess.

So hat sich also in der Tat eine ziemlich gute Lösbarkeit der Teilungaufgabe gezeigt, wenn man nur die Analogieen aus der Lösung ähnlicher Aufgaben auf dem Gebeir der Raumanschauung sinngemäss auf die Zeitgestalten überträgt. Wollte man den ganzen inneren Vorgang mit einem Bilde darstellen, so könnte man die sofortige Gewinnung der richtigen Länge des ersten Teiles bei der Herstellung der Vergleichszeit V etwa mit der proportionalen Verschiebung aller Teilungspunkte einer elastischen Messleine, etwa eines mit Teilungs-Knoten versehenen Gummibandes, vergleichen, die bei der Ausspannung des ganzen Bandes über eine beliebige Strecke V jeden Teilungspunkt sogleich an die richtige Stelle der Raumstrecke gelangen liesse. Da aber die Strecke V auch bei der richtiger einstellenden Vp.I im ganzen von $n = 3$ an aufwärts kürzer wird als N, so gilt doch wieder Meumann's Festellung für seine kleinen Zeiten, dass das Drittel und Viertel gewöhnlich kleiner ausfällt als der gewünschte Bruchteil von N. Nur ist dies bei unseren längeren Zeiten, die auch *ungeteilt* kleiner nachtaktiert werden, höchstens noch Ausdruck einer gewissen Unrichtigkeit der Zeitvorstellung, nicht aber ihrer Unanschaulichkeit.

CAMBRIDGE, ENGLAND: 1887–1937

By Frederic Charles Bartlett, University of Cambridge
Editor, *The British Journal of Psychology*
(Founded in 1904)

I cannot begin this article without expressing, on behalf of British psychologists, our deep sense of indebtedness to *The American Journal of Psychology*, and our admiration for the way in which it has been conducted. The Laboratory from which I write has the good fortune to possess the whole series of its fifty years of publication. The volumes have been well and truly used. Some of them contain the pencilled annotations of generations of approving or of critical students. Again and again the articles have been the inspiration of original investigations. This Journal, with its readiness to keep open its pages for the free discussion of all manner of different views, and its unfailing and continued interest in every phase of human life and experience, has done more than can readily be recorded for the development of psychology in England.

I have wondered many times how I could best contribute to the Jubilee volume, and that I have finally decided to attempt to write a history of psychology in the English Cambridge is partly because it is the story of psychological growth which I know best, partly because it is a record of events and people which has never yet been fully told in public, and partly because the fifty years which are now being celebrated are precisely the period which very nearly covers the whole narrative of Cambridge psychology.

As a matter of fact it is necessary to go a little further back than fifty years for the beginnings. In 1875 James Ward, later to become Professor of Mental Philosophy in the University, presented a dissertation for a Fellowship at Trinity College. It was an essay on *The Relation of Physiology and Psychology*. Later it was privately printed, and one of my cherished possessions is the somewhat tattered copy which was Ward's own. The dissertation was an expository and critical study of experimental psychophysics as developed by Weber and Fechner. Ward used to tell how Henry Sidgwick came to him with shaking head, and said, in his rather hesitating manner: "Y-yes Ward;

it's n-not bad; but r-rather after the st-style of the D-D-Daily T-Tilly-graph." However, it won him his Fellowship, and two years later he and Dr. Venn together proposed to the University that there should be established in Cambridge a laboratory of psychophysics. They were far ahead of Cambridge thought. There was a discussion in the Senate, but a theologically minded mathematician is said—I have never seen the actual report of the proceedings—to have aroused enthusiastic scorn by talking of these men who would "insult religion by putting the human soul in a pair of scales."

If Ward could have had his way Cambridge would have built the first psychological laboratory in the world, though he, then and to the end of his long life, would stoutly have maintained that it dealt not with psychology but with psychophysics. Two years later he tried again, but with the same result. And then, in 1886, Ward published his famous article on Psychology in the *Encyclopaedia Britannica*. There it remained, well-known and immensely influential, yet in a way locked up, for thirty-two years. Very few off-prints were ever circulated. We have one of them at this laboratory now, interleaved, and with annotations in Ward's neat, small hand-writing. Students had to dig out his contribution from its unwieldy setting, haunting libraries to read it and making their own abstracts. It was, as everybody knows, a *system* of psychology, and it was based upon principles never intended to be within the reach of experimental verification or examination. But it was the first important discussion of psychology in England to pay generous and appreciative notice to a mass of experimental work, then only recently performed. It is true that when it was finished Ward tended to retire more and more into metaphysical fastnesses, and the initiative for further real development in Cambridge passed to others. But it was James Ward who first wanted to make Cambridge an active centre of psychological study, who fought the first battles against uninstructed opposition, who by his brilliant writing made people of influence think of psychology as something more than a draughty field of easy amateur investigation. Throughout his life he remained keenly interested in plans for the progress of psychology, and on the whole ready to support them, though often with reservations, and with hesitations and groanings that none who never witnessed them could appreciate.

So far all this was ahead. The notion of building a laboratory for psychology was abroad in Cambridge, but that was all. In 1879 a Classical Scholar, G. F. Stout, had arrived at St. John's College. Ward

attracted and held him, and in 1887, just about the time that the first issue of *The American Journal of Psychology* appeared, Stout became the first Fellow of St. John's to receive honour for psychological studies. Now Ward had recruits in Cambridge: Stout, W. E. Johnson and others.

Thirteen years passed by during which, no doubt, the position was being slowly consolidated, but nothing spectacular happened. In 1891 Ward got the University to make a grant of £50 for the purchase of apparatus for psychological purposes, and a few more similar grants were made in immediately succeeding years. Some of this apparatus still remains: a beautifully constructed Helmholtz double siren, one of the early Hipp chronoscopes, and a few odds and ends, now little but curiosities. Unfortunately most of it has been lost.

In 1897 an entirely new influence appeared on the side of psychology in Cambridge, and from this time on Ward ceased to take the lead in the changes that were to come.

Here, therefore, is the place for me to try to write something about the power and personality of James Ward.

It is difficult. Many men break all bounds of print and page and Ward was one. When I came to Cambridge he was at length free from his contract with the Encyclopaedia, and was seeing through the Press the final proofs of *Psychological Principles*, that book which ought to have been published thirty years before it could be. Gone were the days when he was the only, or even the predominant, psychological force on the scene. He was, indeed, uneasy and apprehensive about the more modern developments, which threatened to submerge the analytic approach by a universal application of experimental and clinical methods. I was a member of the last class for psychology that Ward ever held. It was an unforgettable experience. The class was a small one, but good, and contained several people who have since done much for psychology in this and in other lands. When Ward had doubtfully assured himself that we were worth instructing, he distributed to all of us copies of the complete proofs of his new book. He said we would take it paragraph by paragraph, and we could raise whatever questions we pleased. With simple-minded enthusiasm we made a corporate effort and arrived at lectures armed with many enquiries. Whereupon Ward sadly but firmly pointed out to us that we could not possibly have read what he had written since nearly all these questions were answered "later on." By this time his spell was upon us, as it was set upon nearly everybody who came into personal contact with him. Above everything else we desired him to talk just as

he liked; so we dropped our questions. At this, more sadly, and yet more firmly, he pointed out that we could not possibly have read what he had written, since hosts of unanswered questions abounded there.

After all it was a glorious class. Soon Ward was coming in, his still piercing gray eyes looking us straight in the face, then sitting down and talking as he pleased about what he pleased. Few people can talk as he did. What he said often appeared unpremeditated, and no doubt it was; but it was the fruit of long experience and profound reflection. The right words came naturally and there was no formality. How well I remember him, expounding some onomatopoeic theory of the origin of language, and himself becoming Adam in the Garden of Eden, naming the beasts as they came before him! Ward loved animals.

Once he gave me a great shock by saying: "I model my style on John Bunyan." He believed it, but nobody else would who has met Ward only in books. There the style is rather ponderous and elaborate, over latinised, and with little Greek and German tags here and there. When he talked he was racy, Anglo-Saxon, vivid, simple.

Ward was tall, spare, with magnificent forehead, eyes and nose. When you saw him you could catch the impression of inflexibility and, somewhere beneath, of an unaggressive sadness. He was, suitably with his psychological outlook, crammed full of activity. He could be brusque and say devastating things and follow them up with a disarming twinkle. They told me that I might find him dogmatic, dictatorial, intolerant, even unsympathetic. He had his views. They were often sharp and sweeping, and he did not usually resign them in, or after, argument. Dictatorial in any unhappy sense I never found him, or unwilling to take account of the other side, even though he often thought that the account, fully paid, was trifling. Unsympathetic he never was.

Most people had long forgotten that at the outset of his career Ward had done some very good physiological observations on frog's nerve. He was now alternately a little amusing and terribly irritating in the laboratory. He prowled about, up and down, ill at ease, a bit envious maybe, but far more than a bit disgusted, and inclined to rate all our problems as trivialities. Once I examined with him in a final degree examination. He turned up, armed with knitting needles and ping-pong balls and required the unfortunate candidates, before his eagle and impatient gaze, to demonstrate the operation of the eye muscles. Most of them fumbled and fumbled—for knitting needles do not go easily into ping-pong balls—but one or two with judgment

resigned their task to him and he got so happy and interested in showing them how to answer his own question that he ended up by thinking them to be quite good!

To me he was marvellously kind. Every step I took towards knowing him better was a step towards deeper respect and admiration. "You and I," he said to me several times, "are going different ways." I am afraid it was true. He was old then, eighty and more. Now, when I look back, I think that there are not many men of that age who still are "going" anywhere. But Ward was, to the day of his death.

I must return to the story of events. First we must go back once more to 1886. In that year Dr. J. McKeen Cattell, fresh from his work at Leipsic, came to Cambridge and entered St. John's College as a Fellow Commoner. He carried out experiments on colour vision in the Physics Department, where Clerk Maxwell was reigning, and gave some lectures on the new science of experimental psychology. It is not clear that any further direct developments proceeded from this, but at least a new set of people began to think of psychology with some respect. At St. John's Cattell produced a great impression, so that many stories still pass round concerning him and his activities. One of these is worth putting on record.

After the annual Fellowship election at St. John's it is the custom to hold a College feast. Feasts in Cambridge are, in general, meant to be just feasts, and there is very little speechifying. But on this occasion, after dinner, the Master of the College very briefly, and usually with a kind of premeditated informality, introduces the New Fellows to the Old and to their guests. When I was elected, the late Master, Sir Robert Forsyth Scott, had to find something to say. This is how he did it: "And now we come to Mr. Bartlett, who is a psychologist. A good many years ago an American, Dr. J. M. Cattell, who afterwards became a Professor of Psychology, entered the College as a Fellow Commoner. One night after dinner we all came up as usual to the Combination Room. There was Cattell, gazing earnestly out of a window. Presently we heard him say 'By Jove, that's a pretty girl going through the Court.' So we all crowded to the window to see. There was no girl; and 'That,' said Cattell, 'was a psychological experiment'. "

The next most effective step, however, was taken not by philosophers, or by physicists, but by physiologists. Thus began an association which, though it was soon briefly intermitted, later still became closer, and, let it be hoped, will remain intimate for all time. In 1897 Sir

Michael Foster was Professor of Physiology at Cambridge. Through him a Lectureship was established in Experimental Psychology and the Physiology of the Senses, and Dr. W. H. R. Rivers was asked to accept it. To the immense good fortune of Cambridge he did. There was still opposition, but it was more vocal than serious. How many times have I heard Rivers, spectacles waving in the air, his face lit by his transforming smile, tell how, in Senatorial discussion, an ancient orator described him as a "Ridiculous Superfluity"!

At length Cambridge had its psychological laboratory: one room in the old Physiological Department. There a few students worked hard at the special senses, especially at vision, at various elaborations of psychophysical method—for all his life Rivers retained a profound belief in the value of these as a mode of training—and did some study of fatigue and drugs. In this single room a student who was to play by far the greatest part in the firm establishment of psychology at Cambridge—C. S. Myers—began his life work.

For four years Rivers continued thus, and then, stimulated by him and others, the Moral Science Board stretched out a rather timid and tentative hand again. They "called the attention of the Senate to the need for more adequate accommodation for the teaching of Experimental Psychology," and recommended that a payment of £35 *per annum* for two years be made to the Museums and Lecture Room Syndicate "in consideration of the assignment of certain rooms in a building in St. Tibbs Row as a Laboratory for Experimental Psychology." The Senate, perhaps relieved to be let off so lightly, agreed, and now psychology, having escaped the initial onslaught of the theologians, was established in the odour of sanctity, though I imagine that no psychologist who worked in those dismal rooms had the faintest idea as to the identity of St. Tibbs.

This was in 1901. Meanwhile an event had happened which was destined to alter the whole course of development of psychology in Cambridge. In 1898 Haddon organised his notable anthropological expedition to the Torres Straits. With him went Rivers, Myers, and William McDougall, the last a powerful new recruit who had arrived at Cambridge in 1890, had joined St. John's, and had become the second on the roll of psychological Fellows of this College; Rivers was also and soon after elected to a Fellowship. The expedition won Rivers for Anthropology, and confirmed Myers in his psychological interests. Both returned to England in 1899, Myers to continue medical work in London for another three years and then to return to Cambridge,

and Rivers straight back to Cambridge, but destined for many years now to alternate between journeys into the anthropological field and periods of intense and absorbed writing up of research at the University.

In 1903 psychology made its last move but one. The Cambridge University Press owned a small cottage in Mill Lane, not far from the banks of the Cam. They kindly offered this to the psychologists, at any rate for a time. The psychologists agreed, and the University undertook to make an annual grant of £50 for apparatus and expenses. Thither Rivers, Myers and their students moved, and there, for thirteen years, the psychologists of Cambridge worked.

The cottage is now no more. It has been absorbed by the growing demands of the University Press. Only a few photographs remain. Even from these one can catch a little of the atmosphere, "damp, dark and ill-ventilated," of 16 Mill Lane. Nevertheless the new laboratory was full of enthusiastic workers. A glance through some of the earlier volumes of *The British Journal of Psychology*, which Rivers, Myers and Ward established in 1904, will show what a lot of work, and what good work, was being done there. Perhaps, in the early days of scientific progress, a subject often grows all the more surely if its workers have to meet difficulties, improvise their apparatus, and rub very close shoulders one with another. It is a wonder that Behaviourism did not first grow here, instead of later and elsewhere. The river was near by. Rats abounded. Anybody could observe startle reflexes firing off in all directions. But the preoccupations in those days were with the special senses—colour- and sound-reactions in particular—with optical illusions and perceptual process, with fatigue. This was also the period of Head's and Rivers's famous experiment on the effects of nerve division, though the bulk of these investigations were carried out during week-ends in Rivers's rooms at St. John's.

In 1909 Rivers resigned a part of his Lectureship, and C. S. Myers became the first University Lecturer in Cambridge whose whole duty it was to teach Experimental Psychology. For this he received a stipend of £50 a year. The little band of students increased. The cottage became more and more dilapidated. Already a dream of a laboratory specially built and equipped for the study of psychology had taken firm shape in the mind of Myers. Already, in 1907, the Special Board for Moral Science had again called the attention of the Senate "to the urgent need of more adequate accommodation for the department of Experimental Psychology." The Mill Lane cottage was being used by "fourteen undergraduate students, two advanced students, and three

graduates engaged in research. . . . The building is quite incapable of satisfactorily accommodating even half this number of students." In December 1908 an appeal was launched for money by the Cambridge University Association. The leadership in psychology at Cambridge had passed on to C. S. Myers.

Here, then, I must pause again and try to write my own story of W. H. R. Rivers. It is baffling task. There were two Rivers, the pre-War and the post-War; the pre-War Rivers whose ways of life and thought, whose hopes and fears not many people knew; the post-War Rivers who was here there and everywhere, the heart and spirit of all manner of schemes, writing and working in a feverish hurry, and who, with no official position in the University whatsoever, exercised, perhaps, a more profound influence upon many phases of undergraduate life than any other single person has ever done.

I came to Cambridge in 1909, a few years older than the average undergraduate. No very specialised training was behind me. I was unsure what to do. Like many another in that state of mind I had a hankering after the "big" thing—the Universe and all its problems. First I would do some philosophy, but afterwards? Perhaps more philosophy, or medicine, or psychology of the experimental sort, or anthropology. I went to St. John's. I had read Rivers's lectures on *Kinship and Social Organisation*, and had tried, but failed—I must confess that I have never succeeded since—to read his study of the *Todas*. He had then the somewhat odd post of Director of Moral Science students in the College. A Director was supposed to see his students twice a term, at the beginning to tell them what lectures to attend; at the end to shake them by the hand. At that time of his life Rivers was not always inclined—quite rightly—to take these duties, such as they were, very seriously. However on this occasion I received a note from him, the first of many that were to come in his almost incredibly illegible handwriting, asking me to call on him one day at tea time.

At Cambridge you knock and enter. The room was beautiful, with its brown panelled walls; but nothing else was. It was in an awful muddle, with books and papers and odds and ends of anthropological trophies all over the place. There was a large table in the middle, with a fluffy cloth, and books piled up on it. Then Rivers came out of his inner study, and somehow at once the room became alive, and the things in it were right after all. There he was, rather tall, trim, quick and light in his movements, in navy blue. You got a swift impression of straight, broad shoulders and a jutting chin, and at once of a tre-

mendously alert mind. He shook hands, told me to sit down, sat down himself, said that no doubt I knew what I wanted and how to get it, took off his spectacles, swept his hand across his eyes with perhaps the most familiar of all his gestures, and waited. We did not talk very much at first. He was like a man suddenly come back from somewhere into a world which on the whole he did not like very much. Tea came up from the College Kitchen. He poured me out a very strong cup, snorted audibly when I had little milk and no sugar, and contented himself with sweetened milk and water. I had a bit or two of rather stale bread and butter and a piece of Madeira cake that might almost as well have been made of sawdust. These things are not so trivial as they look. They were part of Rivers at that time, just as the other, and the really vital part of him was the *History of Melanesian Society*. Somehow it came out that I had read a little anthropology, and even that I had heard of Cross-cousin Marriage and the Classificatory System. River's stammer disappeared. The table was cleared of a book or two. For a brief time we pored over complicated diagrams of relationship. Only for a short time: the History was urgent, and out I came again, suddenly to realise that I had been treated, not as an undergraduate, but as an equal.

That was River's way, then and later. It was a great part of his power over men, especially young men. It was not the fruit of judgment. On the whole Rivers had not good judgment about people. Sometimes I think he had no judgment about people at all. When he met you he just believed that you were going to be good, and, if you had it in you, you were vastly more likely to be. If you had not, he found it out, sooner or later, and with his rueful but uncompromising smile he dropped you.

At that time, when he was not away on one of his many expeditions, he worked. He did not drink tea, or coffee, or alcohol. He did not smoke. Sometimes he went up the river, shoving a canoe, as happy as if he was in Melanesia. But he had very few relaxations, and the brown men of the tropic seas were his real world. Often he said to me, "Whatever you do don't let Cambridge get hold of you. They'll try to. But don't get muddled up with administration." He told me that if I wanted to be an anthropologist I must be a psychologist first, and especially I must "know the psychophysical methods."

The History of Melanesian Society was finished. The piles of proofs were through the Press. The two large volumes appeared. The War came. Rivers, back with a bang to psychology and to medicine, went one way and I another. I saw a little of him from time to time during those

years; enough to realise that a change had come. How great a change I never knew till the War was over and we were both back at Cambridge.

He was into everything. Gone was caution. He laid down the law, as the Public Orator once privately remarked, "like Moses;" but as a rule he did not get as upset as Moses might have done if you failed to accept it. He gave lectures everywhere, writing them all out in a tremendous hurry, reading them with a force that mostly carried conviction. He would dash in at all sorts of times with new ideas. Once, in the early morning, he came in, half-dressed, to me, half-dressed, and I had forthwith to sign a document recording certain events that had happened during the night which might, he thought, do something to prove telepathy; but they never did. He abounded in schemes. He said to me often that his real work was finished, and that he would just "let out" ideas and leave them to live or to die. He flung his old advice about not getting mixed up in administration violently behind him. He was elected on the Council, the primary administrative body of the College. He ran the College Book Club. The College invented for him a brand new post called "Praelector in Natural Science Studies," and left it to him to do about it as he pleased. His solution was to get to know personally every science man in the College, and many in other Colleges also. They flocked to his rooms and told him all manner of difficulties which, with a kind of enthusiastic patience, he frequently melted away. His Sunday breakfasts for undergraduates became famous. He founded and ran a new University Society, the Socratics, and brought to it many eminent contemporary novelists, poets and artists. All this time he had no University post. Nevertheless he lectured in Cambridge more than ever before, and crowds came to hear him, hanging upon his words.

All his life he had worked as if driven by some internal urgency, formerly upon the single, absorbing problem of the period, but now in a diffused and mutiple way that must have been intensely tiring. Again and again, coming suddenly in on him, I found him deeply weary. Then, in a moment or two, off would come his spectacles, up would go the hand, in the old, familiar way, across his eyes, as if he brushed all his weariness aside; and he would be alert and confident and full of optimism again. Why, I wondered, this terrific, this insistent hurry?

Then, on a lovely late spring day in 1922, I got the answer. On Saturday, June 3, I went to College, intent on a game of some sort. I met Rivers on the steps of the New Court. He was gay, cheerful, off for one of his quick walks of those days. Near midday on Monday I was passing

St. John's and saw the flag at half-mast. I called at the Porter's Lodge. They said: "Dr. Rivers is dead."

Sightseers were everywhere among the Cambridge Colleges, gazing, joking, laughing; the sun was brilliant; the spring colours were shining. To me, and to many more, it all seemed silly, irrelevant, far away: Rivers was dead. Afterwards I knew that it could have happened at any time, and so he must, at all costs, year by year, do his work before it was too late.

He was a great man. We met him and had no doubt of it. He needed contact to communicate his greatness, which lived in him, and would not wholly go into any other form than himself.

In 1908 the appeal for money was sent out. In 1911 the foundation-stone of the new psychological laboratory was laid. The great bulk of the money had been secured by Myers, and most of it came from his relations and from himself. The Drapers Company of London had given a magnificent benefaction to establish a new physiological laboratory about the same time, and by good-will and great good fortune, they and Professor Langley agreed that the psychology building should become a wing of physiology, but with complete independence as regards internal administration. Two years later the laboratory was formally opened. The persistent, patient and yet brilliant work of many years had reached its partial reward. Only those who have been closely identified with it since can realise the very remarkable foresight with which the building had been planned.

Teaching and research in the new quarters had already begun, and again it was Dr. Myers who organised the former and stimulated the latter. He now became the (unpaid) Director of the Cambridge Psychological Laboratory, and the University provided one Assistant and a workshop mechanic. Myers himself shared an elementary course with Dr. G. Dawes Hicks, and secured Cyril Burt to help him with practical classes. He did nearly all the advanced work himself, with some help now and then from Rivers.

Everything seemed set for the happiest of futures, and then came the War. Myers immediately volunteered for active service, was given a Commission in the R.A.M.C., and went to France. Nearly all of the rest of us went our several ways.

When the War was over psychology had, in a way, to be begun afresh in Cambridge. Undoubtedly the position was easier, for not only had psychologists just proved themselves to be of some use in a crisis, but there were the laboratory and the nucleus of a good teaching

staff. The university made a Readership in Experimental Psychology for Dr. Myers, who thereby received a part, but by no means all, of the recognition he deserved. He and Dr. Rivers succeeded in establishing a Diploma in Psychological Medicine, the necessary money coming in the main, in both cases, from the Medical Grants Fund. First Dr. Prideaux, and later, Dr. J. P. Lowson, now a Professor at Brisbane, came to take over the Medical teaching. About this time also the University agreed to the appointment of an official Demonstrator. For a while, Bernard Muscio, who became Professor of Philosophy at Sydney, and to the grief of all who knew him, died before half his work was done, occupied this post.

Myers knew well that to live psychology must grow. He desired to bring it into closer touch with a wider sphere of activity outside the University. In particular he saw clearly great problems of industrial and social organization in the solution of which psychology must play its intimate part. Neither by geographical position nor by tradition was Cambridge the best place for this kind of development. Moreover no man can go forever patiently at the pace of others, when that pace is very much slower than his own. Myers came back, lectured again, encouraged every possible line of research and put heart into us all. But, as time went on, more and more a part of his mind was elsewhere. He would not leave till everything that could be was well and firmly established, even though what could be was far indeed from what he knew might be. The time came, however, in 1922, and the fact that when he went it was left to me to preserve, and if possible to expand, what he had built—this also was due, not only to the wise and generous guidance which he and others had given me, but immediately to his and to Rivers's great personal influence.

The rest of the story can be told swiftly. In 1921-2, before Myers left and before Rivers died, they had made sure of the establishment of a University Lectureship in Psychopathology. To this, in 1923, Dr. J. T. MacCurdy was appointed.

After the War, a Royal Commission enquired into teaching, research and administration at Oxford and Cambridge. They introduced into Cambridge a system by which all subjects taught were organized into Faculties, and the Psychological Laboratory was at length placed where it properly belonged, with Physiology, Biochemistry and Pathology, as one of a group of "biological sciences." This was in 1926. Very nearly all the subsequent changes have grown out of this one. At the same time a full Lectureship in Psychology was established and was given to Dr.

H. Banister. More and more students came. The research work of the Department grew year by year. Working relationships with other interested Departments became always closer. In 1931 the University decided that it was time to establish a Chair of Experimental Psychology. All this time the Demonstratorship had been maintained. The duties of the Demonstrator became heavier and more responsible, and in 1933, Mr. G. C. Grindley, who was the acting Demonstrator at that that time, became the third full Lecturer at the Laboratory.

Although, since 1926, the Department of Experimental Psychology has, for all purposes of administration and control, ranked with the biological sciences, it is only during the last two years that it has found a place in the Cambridge Natural Science Tripos. It has for many years had its own Part II Tripos, as a section of the Moral Science Honours Degree Examinations; it has, since pre-War days, been one of the subjects which can be offered for the Ordinary B.A. Degree. In 1935 the Natural Sciences Committee in Cambridge recommended that "Psychology and Physiology" should be treated as a joint subject for the Natural Science Tripos, Part II, and this recommendation was accepted. The story of this direction of growth is not finished yet: further changes are bound to come.

Even before the War, the Industrial Health Research Board which had been established by the British Government, had come into close working relationship with research at Cambridge. When, later, it was placed under the control of the Medical Research Council the relation continued and, through various other committees of the Council, became more extensive. The development of Cambridge as a centre for psychological research has, in fact, been tremendously stimulated by the Medical Research Council, and many of the students who have from time to time received financial help from this body are now established in University posts in various parts of England and Scotland. In 1935 the Medical Research Council, with the willing coöperation of the University, decided to establish in Cambridge a Readership in Industrial Psychology for one of their Senior Investigators, Mr. E. Farmer, and for Mr. E. Chambers an Assistant Directorship of Industrial Research. Already this recent development has become an important part of research and teaching in Cambridge.

The post-War period has been one of steady and continuous growth, in an atmosphere of general good-will. Once again the Laboratory has reached the limit of its existing resources. During the last two or three years it has been possible to meet some, but by no means all, of the

increasing demands, only through the generosity of the Professors of Physiology, Sir Joseph Barcroft, and of Pathology, Dr. H. R. Dean, who have found rooms which they could with difficulty spare in their own laboratories for research students from psychology. When the present building was completed, its South wall was left blank of windows and with jutting bricks to proclaim to all that here extension might some day be planned. The University Buildings Syndicate set aside a Southward site and have preserved it untouched since. The time has come. Once more the laboratory must grow.

Such is the story of events in fifty years of psychology at Cambridge. Some day a different history must be written, a history of ideas explored and of research accomplished. Upon these, after all, the real health and vigour of any Department of knowledge must rest. It will be a story containing many names not mentioned in this record; of teachers, research workers and students who have been loyal to the ideals of science and have kept themselves honourably free from the fetters of any fixed, final and dogmatic systematizing. They all, without exception, looking back over fifty years of effort, would pay tribute chiefly to Ward, Rivers and Myers.

Ward had the brilliant ideas, and foresight, the catholicity of interest, the eagerness that welcomes advance. But something robbed him of the final executive drive. Rivers had the sympathy, the infectious enthusiasm, the optimism, the certainty, the power to draw love and respect. But he built with ideas and human beings rather than with more tangible materials which he found somewhat intractable to his touch. Myers had the dreams, and the power to translate them. He had more. When he went to the War he gave me the notes of his lectures. I read them and I read everything that at that time he had written and published. I knew that he had the scientific temper, the sanity, the breadth, the clarity, the insight possessed to the same degree by no other living English psychologist. What, if he had chosen to live for himself, could he have accomplished? What would he have left undone? Psychology in Cambridge was built by all these three, and by others; but if I had to say upon whom its present position and its future possibilities most of all depended, I would, without hesitation, declare: upon C. S. Myers.

ALL-OR-NONE VERSUS GRADUAL ELIMINATION OF HOMOGENEOUS CULS-DE-SAC

By John A. McGeoch, Wesleyan University
Editor, *The Psychological Bulletin*
(Founded in 1904)

The culs-de-sac of a maze may be entered to their full depth each time they are entered at all, or they may be entered completely at first, then less and less completely as practice goes on, until finally they are not entered at all. Elimination is, in the first case, complete or all-or-none; in the second it is gradual, progressive or continuous. The question is whether elimination of individual culs-de-sac proceeds consistently in either of these ways and, if so, which one is a part of the larger problem of the conditions of fixation and elimination.

Hicks and Carr reported in 1912 that rats at first traversed the entire length of a maze alley, but that as training proceeded they identified the alley soon after entrance, and traversed only a part of it.[1] It was implied that a similar result had been obtained with human *S*s. The problem was first given systematic analysis by Peterson in 1917.[2] He ran small groups of rats through 2 mazes and through 2 modifications of each maze in which certain alleys were altered in length. He concluded that the character of the response to a cul-de-sac, as well as the number of entrances, changed with practice. As a rule, the animal ran to the end of a blind during the early stages of training, but as training continued the entrance became less and less complete, until elimination finally took place. This change in the response to the alley was said to be more marked in longer than in shorter blinds, and especially in alleys difficult to eliminate.

Peterson classified entrances as "complete," "half," (*i.e.* any distance between complete entrance and a beginning entrance), and "start" (*i.e.* entrance of the animal's head or the fore part of its body into the alley). The data are given for each group in terms of the numbers of these entrances in class intervals of 10 trials. In some cases all rats are certainly represented in each class interval, and in other cases probably not all are represented. It is, therefore, impossible to make out what the composite course of elimination is in each alley over comparable fractions of the training. The data show, however, an increasing proportion of partial entrances as practice goes on, but both complete and partial entrances continue to be made, insofar as the total records show, until elimination occurred or practice ceased. There is no gradual transition from initial complete entrance through less and less depth of partial entrance, until finally only very short, incomplete

[1] V. C. Hicks and H. A. Carr, Human reactions in a maze, *J. Anim. Beh.*, 2, 1912, 98–125.
[2] J. Peterson, The effect of length of blind alleys on maze learning: An experiment on twenty-four white rats, *Beh. Monog.*, 3, 1917, (no. 15), 1–53.

entrances are made. Entrances of each of the 3 degrees in his classification continue to appear, with few exceptions, throughout training. Peterson accounts for the continuation of complete entrances into the later part of the practice on the basis of confusion effects which carry over from errors made earlier in the run.

Robinson and Wever have shown that rats will enter a blind to a progressively decreasing distance, when they can ascertain visually whether or not the path is open.[3] Albinos are able to do this at a distance of about 18 in., and pigmented animals at about 30 in. Peterson's animals may have utilized visual cues in this way. He recognized the possibility, but believed that it changed in no essential way the theoretical bearing of the results.

The fact that the gross number of partial entrances and the proportion of partial to complete entrances increase as practice proceeds has become well recognized by experimenters. It has, however, sometimes been generalized beyond the facts in statements which imply that complete entrances drop out almost entirely and that the alley becomes eliminated by a progressively decreasing depth of penetration at successive entrances.[4] It has not, moreover, been explicitly confined to animal *S*s but has, by implication, been extended to human *S*s.

Statements about the course of error elimination do not always make clear exactly what is meant. They often seem to mean that the *S* enters to a progressively smaller distance on successive entrances. They may mean, however, that the average depth of entrance in successive fractions of the training decreases. In this case, complete and partial entrances might appear in each fraction, but the proportion of partial entrances might increase and the average depth thus decrease. This could occur even if the depth of the partial entrances in successive fractions were to remain a constant. If the depth of the partial entrances decreases, the average depth would diminish more sharply. The significance of the data is a function of which is meant. A progressively decreasing penetration at successive entrances is a different result from a mingling of complete and partial entrances which yields, nevertheless, a decreasing average depth of entrance.

The problem of the course of the elimination of individual alleys gains additional interest from the fact that it figures as one of the deductions from Hull's habit-family hierarchy.[5] He deduces that an animal *S* "*may be expected, in general, to penetrate the blind alley shorter and shorter distances on successive trials until finally he will not enter it at all.*" The deduction specifically applies to the white rat, but it raises, by implication, the question whether a similar phenomenon occurs in human learning.

Very little data upon the course of individual error-elimination by human *S*s are available. Perkins reports that blinds in a long maze are eliminated primarily by a decrease in the number of entrances rather than by progressive elimination of the depth of individual entrances.[6] Some partial entrances do occur. Of a total of 1050 entrances into the blinds of one maze, 26% were entered part way and usually only to a very short distance. In 2 other mazes, 8% and 22% of all entrances were partial. Nothing is said, however, about

[3] E. W. Robinson and E. G. Wever, Visual distance perception in the rat, *Univ. Calif. Publ. Psychol.*, 4, 1930, 233–239.

[4] Cf. R. S. Woodworth, *Psychology*, 3rd. ed., 1934, 227; and J. Peterson, Aspects of learning, *Psychol. Rev.*, 42, 1935, 1–27, esp. 19. Such statements as these seem to imply more than the available data will support.

[5] C. L. Hull, The concept of the habit-family hierarchy and maze learning, *Psychol. Rev.*, 41, 1934, 33–52, 134–152.

[6] N. L. Perkins, Human reactions in a maze of fixed orientation, *Comp. Psychol. Monog.*, 4, 1927, (no. 21), 1–92.

the course of elimination, whether the partial entrances tended to accumulate toward the end of practice. The maze used was one with a fixed orientation and was explored visually by the *S* through a tracing tube which permitted vision of a small area of the pathway at a time. Results obtained under this condition might well fail to hold for the stylus maze. Crafts and Gilbert have published data which show incidentally that few partial entrances are made after complete entrances into the alleys of a stylus maze have been eliminated.[7]

McGeoch and Peters have found that in the learning of a Warden Multiple-U maze by human *S*s, each alley is entered, particularly during the latter half of the trials required to learn, for a time which is, on the average, nearly a constant for that alley, if it is entered at all.[8] The temporal magnitude of the error does not, during the last half of the trials, decline gradually, but instead is a constant until the error is no longer made. This fact suggests, though it of course does not prove, that the distance of entrance may not decline gradually, and has led to the present experiment.

In the experiment to be described, we have measured the course of elimination of the individual alleys during the learning of a Warden U-maze by human *S*s. We have sought to discover whether elimination takes place primarily by an all-or-none elimination of the entire cul-de-sac or primarily by a less and less depth of penetration into the cul-de-sac, until elimination finally occurs, or by a combination of these two.

Method

The standard maze pattern used was a reproduction of Warden's 10-alley Multiple-U maze, Pattern I.[9] In this maze the alleys are homogeneous in pattern and are all of the same dimensions. The first section of each alley is 40 mm. long, the second section, 24 mm. Since the results might be a function of the length of the alley, 2 other mazes which introduce variations in alley-length have been employed. Each of these mazes duplicated the pattern of the standard maze, but in one of them the longer arm of the odd-numbered alleys was lengthened by 24 mm. and the shorter by 8 mm., and in the other the even-numbered alleys were lengthened by the same amounts. The longer arm was then 64 mm. long and the shorter 32 mm. We shall refer to these two as the odd-lengthened maze and the even-lengthened maze, respectively.

The maze, which was cut in a sheet of fiber board, rested on a glass base and was held firmly in place by a surrounding frame. Between the fiber board and the glass plate ran a band of paper, completely covering the plate. The band was continuous and was inserted through slits in the ends of the framework. By means of a clutch the fiber board could be released from firm contact with the paper so that, at the end of each trial, a fresh writing surface could be drawn through under the maze. During each trial the clutch held the fiber board and the paper firmly against the glass base.

S traced the maze with a pencil-stylus which recorded on the paper each movement made. We have, thus, a graphic record of the path taken by each *S* during every trial. By

[7] L. W. Crafts and R. W. Gilbert, The effect of punishment during learning upon retention, *J. Exper. Psychol.*, 17, 1934, 73–84.

[8] John A. McGeoch and Henry N. Peters, An all-or-none characteristic in the elimination of errors during the learning of a stylus maze, *J. Exper. Psychol.*, 16, 1933, 504–523.

[9] C. J. Warden, Primacy and recency as factors in cul-de-sac elimination in a stylus maze, *J. Exper. Psychol.*, 7, 1924, 98–116.

means of a specially prepared and calibrated stencil, the depth of entrance into each alley, which is the concern of this paper, was measured. A very large number of retracings would have made such a record difficult or impossible to read. Human *S*s do not retrace frequently, even during the first trial, and when they do, retracing movements seldom duplicate the forward-going movements. *E* kept a symbolic record of *S*'s response to each section of the path and, when the direction of *S*'s graphic record was in doubt, the question could be settled by comparison with *E*'s record. There was, thus, no difficulty in identifying the graphic record. For the present purpose, retracings in the culs-de-sac are the only ones which could cause confusion and, even in the early trials, there are seldom more than two retracings per cul-de-sac per trial and frequently there is none.

TABLE I

MEAN SCORES IN LEARNING THREE DIFFERENT MAZES

Maze	N	Trials		Time (sec.)		Cul-de-sac entrances	
		M	σ_M	M	σ_M	M	σ_M
Standard	20	44.3	6.39	1504.2	157.2	149.3	19.5
Odd-lengthened	20	56.5	6.78	1795.4	192.4	173.9	22.2
Even-lengthened	20	41.6	5.41	1408.2	175.1	145.9	22.7

The depth of each entrance into each alley has been measured in millimeters. In addition to the measurements of distance, the usual records in terms of trials, time and number of alley entrances were taken. It is possible to obtain, also, from the graphic records the number of retracings in the true path, the character of *S*'s response in approaching and passing culs-de-sac, and the extent to which the paths taken in succeeding trials differ from each other. Reference will be made later to certain of these data.

The maze was concealed behind an opaque screen of black cloth, through which *S*'s arm was inserted while running the maze. Vision of the screen and of the room was permitted. The maze was placed so that the true path was parallel to the edge of the table facing *S*, and so that the entrance-goal direction was from right to left. The usual maze instructions were given orally and were given twice in succession. *S* was allowed to ask questions about the procedure, if he desired, before practice began.

Twenty college students learned the standard pattern; 20 others learned the same pattern but with the odd-numbered alleys lengthened; and 20 learned the same pattern but with the even-numbered alleys lengthened. Practice was continued to a criterion of 3 perfect runs in succession. All the *S*s relearned after 7 days to the same criterion. The *S*s were students of both sexes at the University of Missouri. None had previously learned a maze of any kind.

RESULTS

Records in the total maze. The total trial, time, and error records have no direct bearing upon the present problem. We present these indices of total difficulty, however, because they furnish measures of the relative difficulty of the 3 mazes. It can be seen from Table I that the standard and the even-lengthened maze are roughly equal in trials and in errors and not far apart in terms of time. The odd-lengthened maze has somewhat higher trial, time, and error scores. We shall make no attempt to interpret these differences, since they do not affect the major issue.

The important thing is that the 3 mazes are not greatly different in difficulty, as thus measured.

Numbers of partial and complete entrances in successive quarters of the trials required to eliminate each alley. If culs-de-sac are eliminated gradually, the number of partial entrances, regardless of their depth, should increase as practice proceeds, while the number of complete entrances should diminish. In order to discover whether elimination takes place in this or in some other way, it is necessary to use the Vincent technique of dividing the practice units into equal fractions and of adding together the amounts learned by the different *S*s in equal fractions of the total practice. In this case, we add separately the numbers of partial and complete entrances in successive fourths. Trials are the practice units usually fractionated in this way. We cannot use, for the present purpose, however, the total trials required to learn the maze, since the individual alleys are eliminated at widely different points during practice and a summation of the partial and complete entrances at successive fourths of the trials required to eliminate the last alley, that is, to reach the criterion for the whole maze, would hopelessly obscure the course of elimination of the individual alleys.

The problem of this experiment can be solved only if we study the course of elimination in each alley. Accordingly, we have found the numbers of partial and complete entrances in each fourth of the trials required to eliminate each individual alley, that is, to reach the point beyond which the alley is not entered again, for each group. The totals represent the number of partial or of complete entrances into each alley in successive quarters of the total number of trials required to eliminate it by each of the *S*s in the group.

In one of the mazes, the odd-numbered alleys were lengthened and, in the other, the even-numbered alleys were lengthened. Since we wish to know the influence of lengthening these alleys upon the course of elimination, we must treat the odd-numbered and even-numbered alleys separately. Separate summations have, therefore, been made of the numbers of partial and complete entrances into these alleys.

The results are given in Table II, together with the percentages which the partial entrances in each case are of the complete entrances. Partial entrances always mean entrances, of whatever depth, into the first arm of an alley. The number of entrances which went beyond the first arm but not to the end of the second arm is so small as to be incon-

sequential.[10] The figures in the last 2 columns are obtained by dividing the mean number of complete entrances into the corresponding mean number of partial entrances.

The number of partial entrances increases, for the most part irregularly, from the first to the fourth quarter, while the number of complete entrances decreases, for the most part, uniformly. In no case, however, are there as many partial as complete entrances.

TABLE II

MEAN NUMBERS OF PARTIAL AND COMPLETE ENTRANCES INTO ODD AND EVEN ALLEYS IN SUCCESSIVE QUARTERS OF THE TRIALS REQUIRED TO ELIMINATE EACH ALLEY, AND THE PERCENTAGES WHICH PARTIAL ENTRANCES ARE OF COMPLETE ENTRANCES

Maze	Quarter	Partial entrances		Complete entrances		Partial ÷ Complete	
		Odd alleys	Even alleys	Odd alleys	Even alleys	Odd alleys	Even alleys
Standard	1	2.63	2.85	26.10	21.19	10.7	13.5
	2	2.16	1.88	17.56	13.19	12.3	14.3
	3	2.85	1.59	15.20	10.44	18.8	15.2
	4	3.61	2.88	13.38	10.48	26.9	27.5
	Total	11.25	9.20	72.24	55.30	15.6	16.6
Odd-lengthened	1	5.91	2.48	28.95	26.77	20.4	09.3
	2	9.32	2.20	16.26	15.80	57.3	13.9
	3	8.16	2.82	9.91	10.89	82.3	25.9
	4	8.84	3.60	10.53	10.74	83.9	33.5
	Total	32.23	11.10	65.65	64.20	49.1	17.1
Even lengthened	1	1.60	2.72	22.33	16.37	07.2	16.7
	2	2.93	3.13	18.34	11.65	15.9	26.9
	3	2.42	4.40	15.97	8.49	15.2	51.8
	4	4.10	6.55	14.76	9.54	27.8	68.7
	Total	11.05	16.80	71.40	46.05	15.5	36.5

The relations between partial and complete entrances appear more clearly in the last 2 columns. There is a steady increase from the first to the fourth quarter in the percentage which the partial entrances are of the complete. This increase appears consistently, save for the insignificant inversion at the third quarter of the trials in the odd alleys of the even-lengthened maze. In the standard maze, the increase is roughly the same in the odd and even alleys; and in the unlengthened alleys of the other mazes, it is closely similar to that in the standard maze. In all of these alleys, the partial entrances in the fourth quarter are between 26.9% and 33.5% of the complete entrances.

In the odd alleys of the odd-lengthened maze and in the even alleys

[10] The mean numbers per *S* for the 10 alleys are 1.35, 0.75, and 0.60 in the standard, odd-lengthened and even-lengthened mazes, respectively. These numbers account for the discrepancies between the totals of the partial plus complete entrances and the mean errors shown in Table I.

of the even-lengthened maze, however, the increase is much greater, until in the last quarter the partial entrances are 83.9% and 68.7%, respectively, of the complete entrances. It should be emphasized that, in spite of the relatively great increase in the percentages in these alleys, the number of partial entrances never reaches equality with the number of complete entrances. There is a uniform trend toward a relative increase in the number of partial entrances, but these entrances remain in the minority throughout. Complete entrances, while they occur with relatively greater frequency during the early stages of elimination, predominate at every stage.

TABLE III

MEAN DISTANCE PER PARTIAL ENTRANCE INTO ODD AND EVEN ALLEYS

Quarter	Standard maze		Odd-lengthened maze		Even-lengthened maze	
	Odd alleys	Even alleys	Odd alleys	Even alleys	Odd alleys	Even alleys
1	28.3	22.7	41.0	22.6	22.9	41.6
2	20.5	23.2	35.1	21.9	20.2	31.4
3	21.9	22.8	34.6	17.6	22.4	32.9
4	20.4	20.4	31.4	17.6	22.1	34.6
Mean of all	22.8	22.3	37.4	19.9	21.9	35.1

It must be concluded, therefore, that both partial and complete entrances occur at every stage of learning and that, although partial entrances tend to increase and complete entrances to decrease as practice proceeds, cul-de-sac entrance throughout practice is more often all-or-none than it is gradual. There is no sign of a shift over from predominantly complete entrances early in practice to predominantly partial entrances late in practice. The two appear together in every quarter of the trials, but the partial entrances are in the minority.

The depth of the partial entrances in successive quarters of the trials required to eliminate each alley. If there is a trend in the direction of gradual elimination, the distance per partial entrance should diminish from quarter to quarter. Such a diminution might occur in the absence of any increase at all in the number of partial entrances or in the proportion thereof; that is, whatever partial entrances are made might be made to less and less depth.

In Table III are shown the results of dividing the mean depth of partial entrance at successive quarters of the trials to eliminate each alley by the corresponding mean number of partial entrances, as given in Table II. These data contain no consistent indication of a trend toward decreasing depth of entrance. The distance is at least slightly greater in the first quarter than in any of the others, save in one instance, and in several cases the distance in the last quarter is less than

in any other, but the decline from the second to the fourth quarter, when it occurs, is too small to be significant. The absolute distance of entrance into the lengthened alleys is greater than that into the unlengthened ones, but the decrease from the second to the fourth quarter is no greater proportionately than it is in the case of the unlengthened alleys. There is here no evidence for any consistent trend toward gradual elimination in terms of the depth of the partial entrances.

It will be noticed that the depth of the partial entrances is, on the average, considerably less than the length of the first section of the cul-de-sac. The Ss have, thus, turned back and emerged from the alley before reaching the end of the first section. Whatever cues are acting, act before the turn is reached. There is, however, little recession of the point at which the cues to the withdrawal response take effect.

The lengthened culs-de-sac are entered farther, in terms of absolute distance, than are the unlengthened alleys, but in proportion to their length they are entered no farther. When we divide the mean distance of all entrances into the unlengthened alleys by the length of the first section of the alley, namely, 40 mm., we obtain percentages of 57.0 and 55.7 for the standard maze, 49.7 for the even alleys of the odd-lengthened maze, and 54.7 for the odd alleys of the even-lengthened maze. When we divide the mean distance of all entrances into the lengthened culs-de-sac by the length of their first section, namely, 64 mm., we obtain 58.4 for the odd alleys of the odd-lengthened maze, and 54.8 for the even alleys of the even-lengthened maze. These percentages are closely grouped around 55.0. Whether the first sections of the alleys are 40 mm. or 64 mm., they tend to be entered, when entered partially, to approximately 55% of their length. If we consider only the depth of entrance in the last quarter, the percentages group themselves around 50. There is still an approximation to entrance to a constant percentage of the total length of the section.

It must be concluded that, save for a small decline from the first to the second quarter of the trials required to eliminate, partial entrances into a cul-de-sac are made to an average constant distance. There is, thus, no gradual decrease of the distance of partial entrances. This average constant depth of entrance approaches a constant percentage of the total length of the first section of the alley.

Cul-de-sac entrances during the first trial. The question at once arises whether the occurrence of partial entrances in every quarter of the trials to eliminate each alley represents a phenomenon which must be taken seriously into account by any theory of elimination, or

whether it may be referred to chance factors. Evidence upon the chance occurrence of partial entrances may be obtained by counting the total number of complete and partial entrances the first time each alley is entered on the first trial. The depth of the initial entrance cannot be a function of prior acquaintance with the alley, and can be only in small degree a function of intra-serial transfer effects. The number and distance of the initial partial entrances give us then a measure of response to the culs-de-sac in the absence of direct learning.

TABLE IV

RECORDS OF THE FIRST ENTRANCES INTO EACH ALLEY IN THE INITIAL TRIAL

	Standard maze		Odd-lengthened maze		Even-lengthened maze	
	Odd alleys	Even alleys	Odd alleys	Even alleys	Odd alleys	Even alleys
No. partial entrances	10	7	6	8	6	10
Partial ÷ Complete	11.1	07.6	06.5	08.9	06.6	11.5
Mean distance	18.0	20.1	39.0	09.9	30.2	42.3

The relevant data appear in Table IV. There is a small number of partial entrances into the alleys of each half of each maze. The percentage, when the number of partial entrances is divided by the number of complete, varies from 6.5 to 11.5. Not every alley was entered on the first trial, so that the total of complete and partial entrances into each alley is sometimes less than 20. The mean distance of entrance varies from 9.9 to 42.3. We have counted as partial entrances a few which went beyond the first arm of the alley and into the second, although not to the end. The mean distance represents, therefore, all incomplete entrances. Partial entrances beyond the first arm of the alley are so few later in learning that they have not been included in the other records.

If we take the percentages given in Table IV as roughly representative of the proportion of partial entrances which may be referred to chance, there can be no doubt that the increase in the percentage of partial entrances from the first to the last quarter of the trials to eliminate is to be considered an integral feature of the learning. The percentages in Table IV are, for the most part, smaller than those in the first quarter of the total trials; and the increases beyond the first quarter are considerable.[11] It must, of course, be recognized that first entrances are not wholly representative of the chance entrances after learning has begun, but they are the best approximation available.

[11] The partial entrances in the entire first trial, which include entrances after retracing, are about the same percentage of the complete entrances as are those in the first quarter of the trials to eliminate.

The increases in the proportion of partial entrances into the lengthened alleys are, especially, too large to be referred to chance. This is particularly true in view of the fact that the initial partial entrances into these alleys are no more frequent than those into the unlengthened alleys. As practice goes on, moreover, the number of retracings in the true path decreases sharply and finally disappears. If we take retracings as an index of disorientation, and as partially due to chance factors, the decline of retracings, coupled with the increase in partial entrances, implies strongly that the partial entrances are a valid feature of the learning.

TABLE V

DATA ON PARTIAL ENTRANCES AT THE LAST ENTRANCE INTO EACH ALLEY DURING LEARNING

	Standard maze		Odd-lengthened maze		Even-lengthened maze	
	Odd alleys	Even alleys	Odd alleys	Even alleys	Odd alleys	Even alleys
Total number	22	18	45	32	26	37
Partial ÷ complete	28.2	21.9	81.8	47.1	35.1	58.7
Mean distance	16.0±2.5	16.2±3.2	26.1±2.9	16.7±2.0	22.4±2.6	30.5±3.1
% possible distance	40.0	40.5	40.8	41.8	56.0	47.7

Distance of entrance at the last entrance into each cul-de-sac. The data of Tables II and III have shown, in terms of the numbers of entrances and the mean distance per entrance during each quarter of the total trials to eliminate each alley, that there is a small increase in the number of partial entrances and an inconsiderable decrease in the distance traversed during these partial entrances. It is possible, however, that division of the total trials to eliminate an alley into quarters is so rough as to obscure a clear tendency toward gradual elimination in the final stages of elimination. Whether this is true may be determined by finding the number and depth of the partial entrances at the last entrance into each alley, that is, on the final entrance before complete elimination. We may legitimately suppose that the final entrance should give the maximal number and minimal depth of partial entrances if elimination is gradual.

Table V gives these data, together with the percentage which partial entrances are of complete entrances, and with the percentage which the distance of entrance is of the total length of the first section of the alley. Since 5 alleys and 20 *S*s are represented in each entry in the first row (total number), there are 100 entrances, all of which could be partial. The actual number of partial entrances may also be regarded, therefore, as the percentage of all entrances which are partial.

The percentages which the numbers of partial entrances are of the numbers of complete entrances do not differ greatly from the corresponding percentages for the last quarter of the total trials to eliminate each alley. The direction of the differences is by no means uniformly toward a decrease from the percentage for the last quarter to that for the last trial. The mean distance of entrance is somewhat less at the last trial than for the last quarter, and the percentage which this distance is of the total possible distance (the length of the first section) is less. The data on the final entrance into each alley show, thus, no sign of a greater tendency toward partial rather than complete entrance than does the entire last quarter of the trials, and only a small amount of decrease in the depth of those entrances which are partial.

TABLE VI

RECORDS OF ENTRANCES DURING RELEARNING

	Standard maze		Odd-lengthened		Even-lengthened	
	Odd alleys	Even alleys	Odd alleys	Even alleys	Odd alleys	Even alleys
No. entrances, 1st trial	23-1	15-1	26-8	15-1	36-3	15-5
No. entrances, last trial	35-16	36-8	31-31	28-14	40-13	27-14
Partial ÷ complete, last trial	45.7	22.2	100.0	50.0	32.5	51.9
M. dist. partial entrs.	18.4	21.4	30.7	15.8	18.2	27.4
% possible distance	46.0	53.5	47.9	39.5	45.5	42.8

Partial entrances during relearning. A continuation of our observations during relearning after 7 days provides an important check upon and extension of the conclusions reached from the data on learning alone. The bearing of the relearning records can be sufficiently shown by an analysis of the entrances during the first relearning trial and at the last entrance made into each alley in relearning. The first row of data in Table VI (No. entrances, first trial) gives, first the number of complete entrances into each group of alleys, and after the dash, the number of partial entrances. Thus, there are 23 complete entrances and 1 partial entrance into the odd alleys of the standard maze. The second row gives the corresponding data for the last entrance into each alley, and the third the percentage which partial entrances are of complete. The fourth and fifth rows, respectively, give the mean depth of the partial entrances at the last entrance, and the percentage which this depth is of the possible distance, that is, of the length of the first section of the alley.

If there is a strong tendency toward gradual elimination, it should show itself at the first relearning trial in the form of a large number of partial entrances into those alleys which are entered at all. At the

first relearning trial, however, complete entrances predominate and partial entrances are few in number.[12] Inspection will reveal that the percentage of partial entrances is much smaller than at the last trial before elimination during learning. The number is so small that a statement of the average depth of entrance means nothing.

If there is a strong tendency toward gradual elimination, it should show itself still more clearly at the last entrance into an alley during relearning than at the corresponding point during learning, because at the point of elimination during relearning there has been still more practice and elimination is occurring for the second time. The second row of the table records a smaller absolute number of partial entrances than at elimination during learning. The percentages in the third row are sometimes higher and sometimes lower than during learning. It should be noted that not all alleys are entered during relearning, and the number of partial entrances under each heading cannot be subtracted from 100 to give the number of complete entrances.

There is, thus, no marked change toward a dominance of incomplete entrances. The distance of entrance, when entrance is partial, is sometimes less and sometimes greater than at the last entrance during learning. The percentages which the distances of entrance are of the length of the first section of the alley, vary between 39.5 and 53.5 and are, thus, in the vicinity of those at the last entrance during learning.

The relearning data exhibit no clearer tendency toward gradual elimination than do the learning data. The evidence from relearning supports that obtained in learning and extends the conclusions to cover relearning to a criterion after 7 days.

The influence of length of alley. The data already presented have made it clear that the increase in number of incomplete entrances as practice proceeds is a function of the relative length of the alley. In the odd alleys of the odd-lengthened maze, the percentage of partial entrances increases from 20.4 in the first quarter to 83.9 in the fourth, while the corresponding increase in the even alleys of the even-lengthened maze is from 16.7 to 68.7. These are much larger increases than occur in any case with alleys of the standard length.

[12] It might be argued that this predominance of complete entrances at the first relearning trial has no direct bearing on the problem because forgetting may be a function of the different complete-partial entrance relations in different alleys. Individual alleys do differ in this respect, but examination of the data reveals no clear relation between depth of entrance at the last entrance during learning and at the first relearning trial, and no clear relation of any kind which would support the hypothesis stated. If elimination is gradual, one might expect forgetting to affect depth of partial entrance and, perhaps, to produce more partial entrances, but hardly to reduce them.

In spite of the greater increase in frequency of partial entrances into the lengthened alleys, the distance of entrance bears approximately the same relation to the total length of the alley as do the partial entrances into the unlengthened alleys. Depth of partial entrance approaches a constant relation to the total length of the section, whether the alley is standard or lengthened. The percentage of incomplete entrances among the last entrances into each alley, during either learning or relearning, is not consistently greater in the case of the lengthened than of the standard alleys.

It must be concluded that the degree of increase in the number of partial entrances is a function of the length of the alley, being much greater when the alleys are lengthened as in this experiment, but that the depth of partial entrance, relative to the length of the alley-section, is not a function of the length of the alley.

Odd alleys versus even alleys. The entrances into the odd and into the even alleys have been kept separate throughout, in order to permit direct comparisons between the standard maze and the lengthened alleys of the two others. It will have been noticed that the odd alleys of the standard maze were entered, both completely and partially, oftener than were the even alleys. In the odd-lengthened maze, this is accentuated in the case of partial entrances, but in the case of complete entrances no differences appear between odd and even alleys. In the partial entrances of the even-lengthened maze, this is reversed and more partial entries are made into the lengthened even alleys, while fewer complete entries are made into these alleys.

These differences between odd and even alleys present a problem which lies outside the scope of the present paper. The facts upon the course of elimination stand, quite aside from any interpretation of the differences between alleys. We have mentioned these odd-even effects because they substantiate the conclusion that number of partial entrances is a function of alley-length. Changes in alley-length produce a shift in the odd-even relations which obtain in the standard maze, and this fact makes the more certain the relation between length and frequency of partial entrance.

Data on a second group in the standard maze. A second group of college students learned the standard maze under conditions identical with those of the group already discussed, but without graphic recording of the path taken. *E* took a complete symbolic record of the path, and this record is adequate for many phases of the problem. The *S*s, all of whom were men, were students at Wesleyan University. The data

yielded by this method of recording are the same as those already discussed, save for the absence of measurements of distance. *E* recorded, however, whether a partial entrance went to the end of the first section of the alley or stopped short of it.[13]

The data corroborate those of the standard group already discussed, and there is no need to reproduce them. There is the same decrease in number of partial entrances from the first to the fourth quarter, and very closely similar percentages of partial entrances. This verification by data from a different group of *S*s leaves little doubt that the results already discussed, for the standard maze and by implication for the others, may be taken as characteristic of the performance of college students.

Summary and Discussion

We have found no evidence of a uniform shift from complete to partial entrance as learning proceeds, and no evidence of a progressively decreasing depth of partial entrance in the case of those entrances which are incomplete. Partial entrances occur in the first trial and increase in number from the first to the fourth quarter of the trials to eliminate each alley. At no point, however, are they as numerous as are complete entrances, even at the final entrance before elimination.

Complete entrances predominate throughout practice. If we translate the data into percentages which complete entrances are of all entrances at the last quarter, we find for alleys of standard length a range between 74.9% and 78.7%. Approximately three-quarters of all entrances in the last quarter are complete. The corresponding percentages for the lengthened alleys are 54.3 and 59.3. Here a smaller percentage of the entrances are complete, but still there are more than 50%. Between 68% and 82% of the final entrances into each alley of standard length are complete, and between 55% and 63% of those into the lengthened alleys.

Cul-de-sac entrance and cul-de-sac elimination are, thus, predominantly complete or all-or-none in a maze the alleys of which are of a constant length, and more than half all-or-none in two mazes in which alternate alleys are lengthened. Partial and complete entrances are, however, distributed irregularly throughout practice and the proportion of the partial entrances increases with practice, though without decrease in the average distance of entrance. Elimination is both

[13] The writer is indebted to Dr. Grace O. McGeoch for aid in running the *S*s in this and in the preceding experiments.

all-or-none and partial, though not progressive. These are the facts which require interpretation in terms of a general theory of error elimination.

Peterson interpreted his results in terms of his earlier stated *completeness of response theory*. According to this theory, when complex serial stimuli act on an animal to bring about a series of responses, the last of which is 'successful,' the effects of the earlier stimuli hold over so that, in a sense, all are acting simultaneously. The resulting response is the most complete and consistent one. Let the true path of a Warden maze be designated as A and B on either side of a cul-de-sac entrance, and let the cul-de-sac be designated X. As the animal comes down A, it has tendencies both to go ahead to B and to turn aside to X. Suppose that it enters X and is blocked. When it emerges, the tendency to go along B, though weaker, is still present and is a determiner of the forward turn from the alley exit. If B had been chosen first, the tendency to enter X would have been weakened. When the goal has finally been reached, the tendencies to enter the alleys, which still persist, are 'relaxed,' since the act is now complete.

Fixation and elimination occur as a result of the holding over or overlapping of response tendencies. Activity drains, as it were, into the tendency toward the complete and unimpeded act. Tendencies to make errors are gradually sucked in toward the main, complete response tendency, until having passed through a stage of hesitation or 'gesture' toward the alley, the error tendencies are eliminated completely. This theory leaves many aspects of elimination unaccounted for, but we shall consider only the extent to which it covers the facts of the course of elimination.

Granting the assumed overlapping of responses and the tendency of blocked responses to be sucked in toward the complete response, we should expect an increasing dominance of partial entrances to a progressively decreasing distance, until finally all entrances are partial and then eliminated. We may grant that this would not actually occur because of confusion resulting from chance entrances into alleys after they had been for some time nearly or quite eliminated, a condition which Peterson offers to account for the appearance of complete entrances late in learning. Certainly, however, the hypothesis implies a steadily increasing dominance of partial entrances.

Such an increasing majority of partial entrances does not appear in human learning of the U-maze. Incomplete entrances increase, but they never reach an equality with complete entrances, and the dis-

tance of entrance of the partial entrances does not decrease greatly as practice goes on. These facts contradict the expectations from the completeness of response hypothesis. Were one concerned to make the hypothesis fit the facts of this experiment, it might be done by the addition of a number of special assumptions. It seems better, however, to attempt to look at the facts in other ways and with fewer assumptions.

The data also contradict for human *S*s the deduction made by Hull that, in eliminating goalward-pointing blinds, the animal *S*s will turn and emerge before reaching or even seeing the end, and that the depth of such partial entrance will decrease with practice. The second section of each alley in the Warden maze points goalward; the first section is at right angles to the line connecting start and goal. It should be remembered, also, that the *S*s who made partial entrances seldom entered the goalward-pointing section during any quarter of the practice. Hull's deduction applies specifically only to the behavior of the white rat. The deduction flows from the conceptual framework of the habit-hierarchy and conditioned response principles, the presentation of much of which suggests that it is thought to apply to a wider setting than the responses of the rat. The departure of these results for human *S*s from the expectation for rats is, therefore, worth pointing out.

Having as little regard as possible for special hypotheses of error elimination, let us inquire what conditions are necessary to bring about the obtained results. We may take it as fact that the human *S* is motivated to eliminate culs-de-sac and to find and fixate the true path. He has consented to serve as *S* and the instructions have specifically informed him that the maze contains blinds which are to be eliminated and a true path which is to be fixated. The sensory consequences of coming to the end of a blind alley, from which there is no escape save by retracing, stimulate the retracing. They are the information that he has entered a cul-de-sac, the primary stimulus to the return from it. They constitute in the first instance the sole distinction between cul-de-sac and true path. They are obtained only by complete entrance into the blind.

When *S* turns back before he reaches the end of an alley, he must, presumably, have received some stimulus which is symbolic of the sensory consequences of hitting the end of the alley. The turning back is, then, an *anticipatory response*, in that it anticipates the response to the

actual contact with the end.[14] When *S* enters completely, he is dependent on the actual stimulation of the end; symbolic cues do not operate. We have found that complete and partial entrances are distributed throughout practice; sometimes *S* must wait for the contact stimulation, sometimes he responds to symbolic cues, but he must wait for the stimulation in many more cases than not.

There is, however, a uniform tendency toward an increasing number of partial entrances. It is relatively small and in most alleys it is irregular. The fact of the increase implies that the symbolic cues are gaining effectiveness. That they gain so slowly and that they never reach dominance require consideration, in view of the fact that the human *S* acquires readily so many responses to symbolic cues. It may be that the symbolic cues in the maze are not readily identifiable by *S*, that is, they are not easily discriminable. It may be that *S* is not set to look for them, or that he is set to look for something else, and that the set determines their failure to act, or that the connection of the symbolic cues with turning back before the end of the cul-de-sac is reached may be readily inhibited.

What symbolic cues to anticipatory return from the alley might gain effectiveness as practice proceeds but always operate spasmodically and irregularly? We may rule out sensory cues from the construction of the path itself. The alleys were smoothly cut, the floor was uniformly smooth, and the presence of cues from either sides or floor is highly improbable. The *E*s could detect no such cues, the *S*s in this experiment report none, and the *S*s in another experiment in which verbal reports were taken reported none. The fact that different *S*s turned back at different points and that the same *S* would turn back at different points in successive trials further support the absence of response to direct differential stimulation from the maze.

We are left with the conclusion that the returns must have been in response to intra-organic cues. The most reasonable inference is that the cues are somaesthetic. Traversing the maze inevitably yields somaesthetic stimulation, although *S* may not rely upon it entirely for guiding cues. He may verbalize, but the verbalization, 'turn right,' or the counting of the turns, does not readily provide a basis for the anticipatory response of turning back before reaching the end of the alley. Verbalization is more likely to be correlated with all-or-none

[14] It should be understood that this discussion applies only when *S* turns back oftener than chance would lead us to expect. We have shown that the partial entrances are much more frequent than chance by any reasonable estimate which can be made of chance.

entrance and elimination. The same is true of the visual imagery mode of attack. The *S*s may use and probably do use all three modes, but the somaesthetic is the more probable determiner of partial entrances.

How may somaesthetic cues act to bring about the partial entrances made in this experiment? Again let A and B designate the true path on the starting and goalward sides of the entrance to an alley, and let X designate the alley. At each entrance into X, the somaesthetic stimulation of moving down A is followed by the blocking at the end of X. Without regard to an hypothesis of the primary conditions of the elimination, the somaesthetic stimulation at A and at the end of X must be considered to be essential features of the elimination. Neither stimulation can be regarded as disparate or atomistic; each may be thought of as being a gradient, the limits of which are not sharply identified by *S*. When he turns back before reaching the end of X, it must be because he has reached a point on the gradient of stimulation which is above the limen as a stimulus to the returning response. The distance traversed before this point is reached varies with the *S* but it is, on the average, less than the total depth of the first section of the alley.

Whether an *S* will enter an alley in a given run, after the first, or to what depth, is a function, also, of a large number of intra-serial conditions. A shift in or loss of orientation, shifting anticipatory intrusions, and inhibitions may account for some of the variability.

There is no progressive elimination in terms of the distance of partial entrance, because *S* must, if he is to turn before the end, enter far enough to reach the threshold region of the gradient. The absolute distance of entrance may vary with the length of the alley because the threshold is a function of alley length and probably of the total depth of the alley relative to the depth of other alleys. The approximate constancy of the percentage which the mean distance of partial entrance is of the total depth of the section of the alley supports this. With either length of alley, however, the connection between the area of the gradient which sometimes stimulates return and the turning response is not fixed, although it gains with practice.

The lack of stable connection between them is reasonable, when one remembers that somaesthesis is the most ubiquitous mode of stimulation in maze learning and that in a homogeneous maze there is no objective basis for differentiation in somesthetic terms between the culs-de-sac and the true path. The long sections of the true path are equal in distance to the long sections of the alleys and the shorter, connecting

sections of the true path are only 5 mm. shorter than the short sections of the alleys, a distance which is probably below the limen of discrimination under these conditions. Whatever the primary conditions of connection between the entrance gradient and the return, it is readily understandable that the connection should not always be made.

The relative fragility of the connection is then a function of the difficulty of discrimination of the gradient cue, probably of the frequency of inhibition by responses to similar cues in other parts of the maze, and perhaps because *S* is not set to look for and analyze out the somaesthetic cues to which the turn is made. The three possibilities mentioned earlier may, thus, be actual. When the turning response does occur, it is made, according to this interpretation, as an anticipatory response to an area on a somaesthetic gradient.

An hypothesis which will account either for all-or-none elimination or for partial elimination will account equally well for the other, if the foregoing analysis is sound. Formulation of such an hypothesis goes beyond the reasonable scope of this paper. We have been concerned in this discussion to describe the concomitant occurrence of complete and partial entrances so that the two appear as aspects of an unitary set of events, and so that different principles need not be invoked to account for them.

SITUATION-AND-GOAL SET

By R. S. Woodworth, Columbia University
Editor, *Archives of Psychology*
(Founded in 1906)

No system of psychology can be accepted as adequate if it fails to do justice to two very obvious facts, which may be roughly designated as the space-span and the time-span of behavior. Their genuineness is attested by introspective as well as objective data. The idea intended by the catchword "space-span" is more fully expressed by saying that the organism deals with external objects and events. Dealing with objects is certainly characteristic of man and the higher animals, whose behavior is nicely adjusted to the size, shape, distance, color, weight, and other physical characteristics of the objects. With visual presentation they recognize the same size at different distances, the same shape at different angles, the same physical color under different illuminations; and man at least can recognize hardness, roughness, heaviness, and other physical characteristics through the tactual and kinesthetic senses. We hear "the water running," smell "the fumes from the furnace," feel in our head "the elevator starting or stopping." It is very difficult by introspection to detect the cues of these various objective facts. Binocular disparity, so valuable a cue of distance, is almost unobservable in ordinary vision. The binaural time difference (0.03 ms.) which can serve as a cue that the source of sound is slightly to the right or left of the median plane is far too small to be observed on its own account. Stimulus-characteristics which cannot be directly observed may thus serve as excellent cues of objective facts. Both perception and motor behavior often correspond better with the objective facts than with the stimuli received. In the reception and utilization of a stimulus other factors besides that particular stimulus must be operating, and their locus of operation must be in the brain rather than in the sense organs. Without attempting to evaluate the contribution of experience to the perception of size, distance, shape or color, we simply stress the point that the central factor operates in the process of perception itself and not in superadded higher processes.

Consider for a moment the perception of object color. A white object is seen as white even when it stands in a moderate shadow. Its albedo, the ratio of reflected to incident light, may be 80%, and this albedo is perceived as such, though roughly. If A=albedo, M=intensity of illumination, and S=intensity of the reflected light which reaches the eye, we have the equation, A=S/M. What the observer does, in effect, is to find A when S and M are given. He solves the equation in some direct, non-mathematical way. S is directly given, and M is given by cues of illumination, especially by the depth of the shadow. Let us assume, what is probable, that perception of the situation in general, including the illumination and the shadow, have a slight priority in time over perception of the shaded object; then we can say that the observer is adjusted to M when he receives S. He receives S into a mechanism already adjusted to M, and, therefore, the equation is solved in the very process of reception and A is directly perceived.

There need be nothing mysterious in this notion of a central adjustment for illumination. The iris adjusts itself to the total illumination and so is a factor in determining the amount of light received by the retina from any given object. The retina adapts itself to light and dark. These peripheral adjustments have been shown to be inadequate to account for the facts of color constancy and there must be some important adjustment further back in the optic path.

In the same way we can write an equation connecting the size of an object, its distance from the eye, and the size of the retinal image or stimulus. If the observer is adjusted to the spatial situation in general, including the distance of the object, then retinal size will directly mean objective size.

The fact that these "constancies" are never perfect can mean either or both of two things. The adjustments are imperfect, or there is a sensory impression corresponding directly to the retinal stimulus and not affected by the cerebral adjustment to the objective situation. A white under good illumination may look no whiter or only a little whiter than when seen in shadow, yet it does not look the same. In a good light it gives a stronger or more insistent impression.

I will not attempt here to compare this conception of illumination set with the somewhat similar concepts used by Katz, Gelb, Thouless, Kardos, and Koffka in explaining the facts of color constancy. There are many other examples, such as the different ways in which the alarm clock is heard when one is awake or asleep, which show the dependence of perception upon one's adjustment to the situation. I

am more concerned to show that the same general concept can be fruitfully applied in the field of action and learning, where the time span of behavior creates the theoretical difficulty.

Behavior as a continuous process should, of course, be describable in time units as short as you choose, a second or a millisecond, and what goes on in any millisecond must be the result of the forces, internal and external, acting at just that time. For psychological purposes, we are more apt to attempt a description in terms of stimulus-response units. Such a description is likely to become excessively schematic. For example, how shall we break up the horse's walking into a succession of steps? The fore leg and hind leg steps overlap in time and this overlap shows that there is some unity persisting for longer than a single step.

The eye-voice span in reading aloud affords a well recorded example of overlapping processes in a behavior sequence.[1] The eyes keep several words ahead of the voice, a variable number, about four or five words on the average in a good reader. The vocal response of pronouncing a word follows the foveal stimulus received from that word by an interval of one or two seconds, which is three to six times the reaction-time in reading single familiar words. While one word is being pronounced another word is being seen and intervening words are going through the mill. No real sequence of stimulus-response units can be analyzed out of this behavior; the time-span greatly exceeds that of a single reaction and is evidently determined by a central factor. By holding his voice back in this way, the reader secures more continuity, better phrasing and expression, than if he responded to each word separately with his normal reaction-time.

Much simpler two-phase movements, like striking a blow with a hammer, show a smoothness which is very different from a sequence of two separate movements. For a many-phase movement, observe the flight of a bird from a tree to a bit of food on the ground. He hops off, makes a number of strokes of the wings and passes smoothly into braking with his wings, extending his legs and landing. There is a consistent and persistent steer throughout the performance. If we wish to describe these polyphasic movements as sequences of stimulus-response units we can probably do so, but only on condition that we recognize a persistent steer set up by the original stimulus and con-

[1] G. T. Buswell, An experimental study of the eye-voice span in reading, *Univ. Chicago, Suppl. Educ. Monog.*, 17, 1920, 1–105.

tinuing till the end of the act. This inner steer can be called a goal-set or goal-adjustment.

Just as the concept of situation-set including adjustment to illumination and distance is useful in explaining the perception of objects out there as distinguished from stimuli received, so in describing motor behavior, the notion of goal set is a conceptual means of taking care of the time-span. The goal need be nothing more distant than the outcome of a two-phase movement. Goal seeking in its lowest terms is the following of a consistent temporal pattern under the control of a persisting set.

Situation-set and goal-set are presumably not separate, especially in the more primitive types of behavior. The situation for which one is adjusted usually includes a goal, since the organism is active and engaged in behavior having a time-span greater than that of a simple reaction. If we conceive of an undifferentiated situation-and-goal-set, we avoid the apparent anthropomorphism of attributing rudimentary cognition and goal seeking to animals. The cognition implied in situation-set is not separated from doing, and the goal seeking does not imply ideational anticipation of the goal since the goal is right there in the presented situation.

I will not pause to defend the notion of set in general; it has a long and honorable history. I do not see that set is a non-operational and unmanageable concept in the laboratory. Instructions, situations, and goal-objects presented, and skilled treatment of the subjects can be depended on to arouse and maintain the necessary internal adjustment. A set is a type of implicit response to complexes of stimuli from the environment. The words 'set' and 'adjustment' are not exactly appropriate since set carries a false suggestion of rigidity and adjustment a false suggestion of quietude. In reality these sets are semi-fluid, and are revised from moment to moment in conformity with environmental changes. Adjustment to the environment is a readiness for action. Situation-and-goal-set is an activity in progress.

The utility of this concept can be given a preliminary test by applying it in different fields of psychology and noting whether it introduces clarity into already known results. My own first use of the concept was in 1906, when I was engaged in an introspective study of the immediate antecedents of a voluntary movement.[2] It was, in a sense, a study of the delayed reaction. A short delay was introduced between

[2] R. S. Woodworth, The cause of a voluntary movement, *Studies in Philosophy and Psychology by Former Students of Charles Edward Garman*, 1906, 389 ff.

the decision to make a certain movement and the execution of that movement. Either the subject was told what movement to make or he was given his choice between two or more. He was to report his "condition of mind" in the short interval of delay. There were two types of reports which seemed trustworthy as well as instructive. Both types are included in the report of one subject on his preparation to hit at a mark. First the chosen mark became focal and then retreated to the background, to be succeeded by thought of the chosen hand; this in turn retreated to the background leaving a rather blank condition of readiness, and the actual movement emerged out of this blank condition. The two points of interest are: (1) that the act was specified in a *series* of perceptions or thoughts, the partial specifications holding over after once being made; and (2) that the act once specified by these perceptions or thoughts remained specified during a blank period immediately preceding the movement. In discussing these reports, I said:

> The nervous system may become set or adjusted for a certain act, and remain so for a time without the continuance of clear consciousness of the act. . . . The whole situation, as far as it is known, results in a certain adjustment of the nervous system, so that, for example, acts that would be performed while we are alone are not performed or thought of in public. Each sort of situation produces a corresponding set of the nervous system, and is thus a partial determinant of all the acts that are performed within that situation.
>
> When a man confronted by a novel situation observes this and that feature of it in turn, each new perception leaves behind in the nervous system a temporary adjustment to the feature observed, until the whole situation becomes—not clearly mirrored in any one moment of consciousness—but dynamically represented by the sum or resultant of these partial adjustments. If he then thinks of some change that he can make in the situation and decides to make it, . . . the intention to act adds a new partial adjustment to the existing sum of adjustments.

In revising the above statement to-day, I should want to say that the partial adjustments resulting from perception of features of the situation are not simply added together but are built into a preëxisting framework of adjustment to the total situation. This adjustment is vague and general at first, and becomes more definite and specific as features of the situation are perceived.

In the well-known experiments on delayed reaction, goal set is often visible in the persistent posture of the animal during the interval of delay. When the animal is able to move about during the interval and still go promptly to the right door or container, we have to suppose some kind of super-posture which amounts to a situation-and-goal set. We need not suppose that this set is ideational or symbolic. It

need not in any degree resemble a map which the animal can carry away and consult elsewhere. It may be completely bound to the presented situation. The set is not to be conceived as a sort of replica; it is not representative, though it corresponds to the situation more or less closely. It is only half of a functional unit, the other half being the actual situation. It is analogous to a resonator tuned to a certain pitch but emitting no sound unless vibrations of the correct frequency strike it. It cannot be identical with Koffka's "behavioral environment,"[3] though there is some similarity between the two. For behavioral environment seems to be representative. Koffka says that in exploration it is the behavioral environment that is explored. One can not conceive the situation set as being explored. It incorporates the results of exploration in a dynamic but not a pictorial form.

The situation set, at first a bare framework, is filled in by exploration, including tentative manipulation by which mechanical characteristics of objects are revealed, such as weight, hardness, flexibility. The characters of objects, the make-up of the situation and the happenings that occur there become known, though this 'knowing' is nonideational and wholly bound to the presented situation.

Troland's conception of the retroflex is of great value in understanding both exploratory and goal seeking behavior.[4] The efferent nerve current to the muscles is immediately followed by afferent currents, both from the kinesthetic end organs and also in most cases from distance receptors, since most movements produce perceptible external effects. A behavior segment never terminates in a movement but always in sensory impulses resulting from the movement. Introspectively it is clear that we are almost never interested in the movement itself, as distinguished from the sensations of the movement and especially from the visible or otherwise discernible external consequences of the movement. Muscular contraction in itself is almost never a goal. The human subject is object-directed in his movements as well as in the reception of stimuli. The retroflex therefore consists not simply of sensory impressions but of objective facts perceived. In goal-directed movements the retroflex affords a check on their success or failure. It shows whether one has hit the mark at which the stone was aimed. The *terminus ad quem* of such a bit of behavior is obviously not the muscular contractions in the arm but the perceived objective results.

Exploratory behavior is equally object-directed. A certain spot,

[3] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 27, 37.
[4] L. T. Troland, *The Fundamentals of Human Motivation*, 1928, 215 ff.

within a place to which one is already adjusted in a general way, attracts the eyes, a saccadic movement gives foveal vision of that spot and reveals some character of the spot which is added as a detail to the situation set. Suppose the subject to be a rat, and let the spot be revealed as a hole: he thrusts his snout into it (tentative manipulation) and is blocked; the hole takes on a negative character, it is not a way out, and this negative character of the spot is incorporated into the situation set.

Is it anthropomorphic to credit the rat, inveterate explorer that he is, with an object-directed attitude toward the situation explored? Certainly it would be more anthropomorphic to credit him with a stimulus-directed attitude such as human beings acquire only by training in analysis of sensory complexes. It takes a trained artist to detect the stimulus-colors in a scene, whereas the child, the monkey, and even the fish respond correctly to the color of objects. In lifting a weight it is difficult to detect the sensory cues from skin, joints and muscles, but it is easy to observe the weight of the object. A psychologically unsophisticated rat would almost surely be object-directed. Since we know from the rat's behavior that he explores a locality and becomes adapted to it, it seems safe to assume that his exploration makes him acquainted with the place, his acquaintance being of the non-ideational type which we designate as situation set.

If we are allowed to assume that the animal is object-directed and that exploration builds up in him a practical acquaintance with the presented situation, we shall find that many phenomena of animal learning fall readily into line. The "latent learning" which occurs when a rat runs a maze with no reward in the food box is readily understood as place learning. I will not review the evidence for place learning which I have summarized elsewhere.[5] Maze learning is chiefly place learning. Once the character of each alley has been discovered and sufficiently drilled into the situation set, suitable locomotion follows as a matter of course. Flooding the maze or laming the animal may necessitate a change in the form of locomotion from running to swimming or rolling, but the right path is still followed. In the problem box something more than place has to be learned; some mechanical device has to be manipulated and the character of this device must be discovered by trial manipulations. What is learned is not a particular muscular response to certain stimuli, but the character

[5] R. S. Woodworth, *Psychology*, 3rd ed., 1934, 226–238.

of a certain object. The door button becomes a thing to turn, or the downhanging string a thing to pull, with claws or teeth, it does not matter which.

The law of effect gives some difficulty when the assumption is that a direct connection is formed or strengthened between a given stimulus and a certain motor response, as between the sight of the door button and some specific movement. Since the stimulus-response connection operates before the effect becomes manifest, how can the latter work back on the former? Thorndike has indeed shown that this apparent backward working is physiologically conceivable, but the matter becomes simpler if we assume that the animal is object-directed. While exploring the cage the animal spies the door button; he claws at it and it moves. The fundamental problem in learning lies right here. Does the animal thus learn that the button is movable? I assume that this simple behavior sequence—seeing button, clawing it and getting the retroflex of button moving—yields a momentary acquaintance with that object, an acquaintance which may be strengthened by repetition. If so much as this is granted, the door-opening character of the button can be discovered by further manipulation. The character of goodness attaches for the animal not to his movements in manipulating the button but to the button as an object.

There is no difficulty in finding situation-and-goal set in the conditioned reflex experiment. Adjustment to the laboratory, to the harness, to the experimenter must first be secured. The animal must have explored enough to reach that state of "alert quiet" which Liddell and his coworkers find necessary for the success of the experiment.[6] A metronome begins to tick; unless it is straight in front the animal turns his head and localizes the sound as something objective in the room. When it has ticked a few seconds, and probably before the animal's attention to it has quieted down, a shock supervenes, delivered by an electrode attached to the animal's foreleg. This being the first shock received, the response is violent and continued, but when the episode is over the animal resumes his alert quiet. After several repetitions of the same stimulus-pair, agitation starts at the sound of the metronome; the two stimuli and their responses have become integrated into a single behavior-pattern and are included in its time-span. As the shock is after all not very dreadful and as it cannot be escaped, the whole behavior may become (though not in all

[6] H. S. Liddell, W. T. James and O. D. Anderson, The comparative physiology of the conditioned motor reflex. *Comp. Psychol. Monog.*, 11, 1934, (no. 51), 1-89.

animals) much more matter-of-fact, and we then witness a very definite temporal pattern of behavior. At the sound of the metronome, the animal raises head and ears, stiffens, half flexes the leg which is to receive the shock, receives the shock and flexes the leg completely, then replaces the leg on the floor and visibly relaxes. The animal has developed a definite adjustment to the metronome-shock sequence; he has built into his total situation set a specific adjustment to this feature of the situation; he has learned the 'character' of that metronome. If now an extinction experiment follows, the metronome ticking as before but the shock being omitted, the behavior pattern fades out with repeated trials. The situation has changed in one important particular and the animal has readjusted himself. The metronome has acquired the new character of *not* being followed by a shock.

Our assumption can, it would seem, be subjected to experimental test in this case of the conditioned response. If the animal is object-directed, exceptionally quick conditioning should be obtained if the metronome sound (or buzz or tone) were emitted by the electrode itself. We can also predict that "sensory generalization" could be readily obtained across modalities, if for example a sound and a light were emitted by the same object; and 'differentiation' between two such stimuli should be difficult to establish.

Many interesting questions concerning this whole conception must be deferred for some future occasion. Ideation we can define as dealing with objects not present or situations not present. Since the details of a situation need not all be presented at the same instant (as shown by the delayed response) we can readily believe that a situation set can be reinstated by relatively few cues. We could probably find transitional cases between sets that are rigidly bound to the presented situation and those which are revived in the absence of the situation by verbal cues.

The origin of situation set in the life history of the individual is a matter for investigation. Our assumption is that the object-directed attitude is a fundamental bent of the individual and that it is the main function of the cerebrum. The existence of an external world does not have to be forced upon the child; he is a naïve realist from the start. One of the first characteristics of the world which the child explores is the gravitational, and it is interesting to reflect that the fundamental up–down dimension must be for him, in view of his manifold positions, environmental and not intraorganic. Adults think of their head as up, feet as down, but this is not characteristically true of the child who is

beginning his acquaintance with the world. The child does not 'project' the up–down dimension from his organism into the world, but finds it in his relations with the environment.

The concept of situation-and-goal set lends itself to the study of maladjustment and disorientation. The individual is not an unbiased registering instrument, and his adjustment may be more in tune with his own drives, preferences and goals than with the objective situation.

No system of psychology, as far as I know, has made explicit use of the concept of situation-set. It has seemed absurd to bring the external world into a psychological system except as the source of stimuli and the catch-all of responses. My contention is that the environment must be brought into the center of the system, as a cerebral or organismic set, in order to describe, let alone explain, behavior and thought. Existing systems have not succeeded in their descriptions except by tacitly assuming an object-directed attitude in the subject, even when they ruled this attitude out of their systems. Titchener's judgment in the matter is well known: "It is natural and customary to think, not of mental processes, but of the things and events about us,—while it is, as I believe, absolutely necessary to get rid of things, and to think only of the mental processes, if we are to have a science of psychology."[7] He trained his observers to report only "content" or "process," and not meaning and objective reference. But the observers found it difficult to give a coherent report of their thought processes without telling what they had thought about. Titchener therefore made a concession: meanings might be reported, provided they were sharply distinguished from content.[8]

From a very different angle, certainly, Hunter also formally discards the environment: "Nowhere does the anthroponomist study the subject's environment except as a possible source of stimuli for the subject's behavior."[9] Yet practically all descriptions of behavior are couched in terms of objects and what the subject does with them. Is this type of description adopted simply to avoid the pedantic circumlocution of referring continually to the detailed stimuli and muscular contractions, or is there not a tacit and inevitable assumption that the animal is dealing with the environment?

It might seem that situation-set would be an acceptable concept to

[7] E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-Processes*, 1909, 145 ff.
[8] Titchener, Description vs. statement of meaning, this JOURNAL, 23, 1912, 165–182.
[9] W. S. Hunter, Anthroponomy and psychology, *Psychologies of 1930*, 1930, 297.

the Gestalt psychology; but it is really inconsistent with emphasis on the fundamental rôle of the stimulus-pattern or of organization in the visual field. The common man as he looks about is not interested in configurations but in objects, not in the visual field but in the objective field. We scarcely realize how little stability or practical significance inheres in the stimulus-pattern. Let us permit our observer to remove his head from the chin rest and to move freely about the room; or let the observer be a child on a visit, playing in a strange yard. As he inspects the interesting objects from various angles, and runs all around the yard, his visual pattern is changing from moment to moment. The succession of his visual fields would make a surprising motion picture. But this continual flux does not confuse the observer. He soon knows the location of the important objects and is quite at home. What he has built up is certainly not a stable visual configuration; it is a set for the objective situation.

A fundamental principle of Gestalt psychology, as I understand, is that organization in a sensory field is only secondarily controlled by central factors; fundamentally it is impressed upon the field by the stimulus-pattern. A situation-set, however, is already present whenever any stimulus-pattern is received. The stimulus-pattern must fit into the framework provided by the situation-set, though it may to be sure cause that set to be revised. The central object-directed factor is primary.

The foregoing discussion is offered as a contribution to a movement in progress. Many psychologists have expressed somewhat the same ideas in different form. I hesitate to make a hasty list of them for fear of omitting some who should certainly be included.

THE FUNCTIONAL AUTONOMY OF MOTIVES

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(Founded in 1906)

For fifty years this JOURNAL has served both as a rich repository for research and as a remarkably sensitive record of the psychological temper of the times. These two services are of great historical value. Since there is no reason to doubt that *The American Journal* will continue to hold its position of leadership in the future, one wonders what new currents of psychological interest its pages will reflect in the coming half-century. With what problems will psychologists be chiefly concerned? What discoveries will they make? What types of scientific formulation will they prefer?

To predict at least one of these trends accurately requires no clairvoyance. On all sides we see the rising tide of interest in problems of personality. Up to a few years ago the somewhat segregated field of clinical psychology alone was concerned; but now theoretical and experimental psychology are likewise deeply affected. As never before the traditional portrait of the "generalized human mind" is being tested against the living models from which it is derived. As compared with particular minds it is found to lack locus, self-consciousness, organic character, and reciprocal interpenetration of parts, all of which are essential to personality. Unless I am greatly mistaken the coming half-century will see many attempts to replace the abstract datum (mind-in-general) with the concrete datum (mind-in-particular), even at the peril of a revolutionary upset in the conception of psychology as *science*.

Some of the best known definitions of psychology formulated in the past fifty years have given explicit recognition to the individuality of mind—that is, to its dependence upon the person. But these definitions have not as yet noticeably affected the abstractive tendency of psychological research—not even that of their authors. Wundt, James, and Titchener serve as examples. The first wrote: "*It* [psychology] *investigates the total content of experience in its relations to the subject.*" The second: "*Psychology is the science of finite individual minds;*" and the third:

"*Psychology is the study of experience considered as dependent on some person.*" None of these authors developed his account of mental life to accord with his definition. It is as though some vague sense of propriety guided them in framing their definitions; they *knew* that mind (as a psychological datum) exists only in finite and in personal forms. Yet their historical positions—the spirit of the times in which they worked—prevented them from following their own definitions to the end. Had any one of them done so, the psychology of personality would have had early and illustrious sponsorship.

In line with what I regard as a certain development in the psychology of the future I venture to submit a paper dealing, I think, with the one issue that above all others divides the study of mind-in-general from the study of mind-in-particular. Motivation is the special theme, but the principle involved reaches into every nook and cranny of the evolving science of personality.[1]

Two Kinds of Dynamic Psychology

Any type of psychology that treats *motives*, thereby endeavoring to answer the question as to *why* men behave as they do, is called a *dynamic psychology*. By its very nature it cannot be merely a descriptive psychology, content to depict the *what* and the *how* of human behavior. The boldness of dynamic psychology in striking for causes stands in marked contrast to the timid, "more scientific," view that seeks nothing else than the establishment of a mathematical function for the relation between some artificially simple stimulus and some equally artificial and simple response. If the psychology of personality is to be more than a matter of coefficients of correlation it too must be a dynamic psychology, and seek first and foremost a sound and adequate theory of the nature of human dispositions.

The type of dynamic psychology almost universally held, though sufficient from the point of view of the *abstract* motives of the generalized mind, fails to provide a foundation solid enough to bear the weight of any *single* full-bodied personality. The reason is that prevailing dynamic doctrines refer every mature motive of personality to underlying original instincts, wishes, or needs, shared *by all men*. Thus, the concert artist's devotion to his music is sometimes 'explained' as an extension of his self-assertive instinct, of the need for sentience, or as a symptom of some repressed striving of the libido. In McDougall's

[1] What follows is drawn in part from Chapter VII of my forthcoming book, *Personality: A Psychological Interpretation*, 1937.

hormic psychology, for example, it is explicitly stated that only the instincts or propensities can be prime movers. Though capable of extension (on both the receptive and executive sides), they are always few in number, common in all men, and established at birth. The enthusiastic collector of bric-a-brac derives his enthusiasm from the parental instinct; so too does the kindly old philanthropist, as well as the mother of a brood. It does not matter how different these three interests may seem to be, they derive their energy from the same source. The principle is that a very few basic motives suffice for explaining the endless varieties of human interests. The psychoanalyst holds the same over-simplified theory. The number of human interests that he regards as so many canalizations of the one basic sexual instinct is past computation.

The authors of this type of dynamic psychology are concerning themselves only with mind-in-general. They seek a classification of the common and basic motives by which to explain both normal or neurotic behavior of *any* individual case. (This is true even though they may regard their own list as heuristic or even as fictional.) The plan really does not work. The very fact that the lists are so different in their composition suggests—what to a naïve observer is plain enough—that motives are almost infinitely varied among men, not only in form but in substance. Not four wishes, nor eighteen propensities, nor any and all combinations of these, even with their extensions and variations, seem adequate to account for the endless variety of goals sought by an endless variety of mortals. Paradoxically enough, in many personalities the few simplified needs or instincts alleged to be the *common* ground for all motivation, turn out to be completely lacking.

The second type of dynamic psychology, the one here defended, regards adult motives as infinitely varied, and as self-sustaining, *contemporary* systems, growing out of antecedent systems, but functionally independent of them. Just as a child gradually repudiates his dependence on his parents, develops a will of his own, becomes self-active and self-determining, and outlives his parents, so it is with motives. Each motive has a definite point of origin which may possibly lie in instincts, or, more likely, in the organic tensions of infancy. Chronologically speaking, all adult purposes can be traced back to these seed-forms in infancy, but as the individual matures the tie is broken. Whatever bond remains, is historical, not functional.

Such a theory is obviously opposed to psychoanalysis and to all other genetic accounts that assume inflexibility in the root purposes and

drives of life. (Freud says that the structure of the Id *never* changes!) The theory declines to admit that the energies of adult personality are infantile or archaic in nature. Motivation is *always* contemporary. The life of modern Athens is *continuous* with the life of the ancient city, but it in no sense *depends* upon its present "go." The life of a tree is continuous with that of its seed, but the seed no longer sustains and nourishes the full grown tree. Earlier purposes lead into later purposes, and are abandoned in their favor.

William James taught a curious doctrine that has been a matter for incredulous amusement ever since, the doctrine of the *transitoriness of instincts*. According to this theory—not so quaint as sometimes thought—an instinct appears but once in a lifetime, whereupon it promptly disappears through its transformation into habits. If there *are* instincts this is no doubt of their fate, for no instinct can retain its motivational force unimpaired after it has been absorbed and recast under the transforming influence of learning. Such is the reasoning of James, and such is the logic of functional autonomy. The psychology of personality must be a psychology of *post-instinctive* behavior.

Woodworth has spoken of the transformation of "mechanisms" into "drives."[2] A mechanism Woodworth defines as any course of behavior that brings about an adjustment. A *drive* is any neural process that releases mechanisms especially concerned with consummatory reactions. In the course of learning, many preparatory mechanisms must be developed in order to lead to the consummation of an original purpose. These mechanisms are the effective cause of activity in each succeeding mechanism, furnishing the drive for each stage following in the series. Originally all these mechanisms were merely instrumental, only links in the long chain of processes involved in the achievement of an *instinctive* purpose; with time and development, with integration and elaboration, many of these mechanisms become activated directly, setting up a state of desire and tension for activities and objects no longer connected with the original impulse. Activities and objects that earlier in the game were *means* to an end, now become *ends* in themselves.[3]

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[2] R. S. Woodworth, *Dynamic Psychology*, 1918. Equivalent assertions are those of W. Stern concerning the transformation of "phenomotives" into "genomotives" (*Allgemeine Psychologie*, 1935, 569), and of E. C. Tolman regarding the "strangle hold" that "means-objects" acquire by "setting up in their own right" (Psychology versus immediate experience, *Phil. Sci.*, 2, 1935, 370).

[3] "The fundamental drive towards a certain end may be hunger, sex, pugnacity or what not, but once the activity is started, the means to the end becomes an object of interest on

Although Woodworth's choice of quasi-neurological terminology is not the best, his doctrine, or one like it is indispensable in accounting for the infinite number of effective motives possible in human life, and for their severance from the rudimentary desires of infancy. Further discussion of the operation of the principle and a critique of Woodworth's position will be more to the point after a review of the evidence in favor of the principle.

Evidence for Functional Autonomy

We begin in a common sense way. An ex-sailor has a craving for the sea, a musician longs to return to his instrument after an enforced absence, a city-dweller yearns for his native hills, and a miser continues to amass his useless horde. Now, the sailor may have first acquired his love for the sea as an incident in his struggle to earn a living. The sea was merely a conditioned stimulus associated with satisfaction of his 'nutritional craving.' But now the ex-sailor is perhaps a wealthy banker; the original motive is destroyed; and yet the hunger for the sea persists unabated, even increases in intensity as it becomes more remote from the 'nutritional segment.' The musician may first have been stung by a rebuke or by a slur on his inferior performances into mastering his instrument, but now he is safely beyond the power of these taunts; there is no need to compensate further; now he loves his instrument more than anything else in the world. Once indeed the city dweller may have associated the hills around his mountain home with nutritional and erotogenic satisfactions, but these satisfactions he now finds in his city home, *not* in the mountains; whence then comes all his hill-hunger? The miser perhaps learned his habits of thrift in dire necessity, or perhaps his thrift was a symptom of sexual perversion (as Freud would claim), and yet the miserliness persists, and even becomes stronger with the years, even after the necessity or the roots of the neurosis have been relieved.

Workmanship is a good example of functional autonomy. A good workman feels compelled to do clean-cut jobs even though his security, or the praise of others, no longer depends upon high standards. In fact, in a day of jerry-building his workman-like standards may be to his economic disadvantage. Even so he cannot do a slipshod job. Workmanship is not an instinct, but so firm is the hold it may acquire on

its own account" (Woodworth, *op. cit.*, 101). "The primal forces of hunger, fear, sex, and the rest, continue in force, but do not by any means, even with their combinations, account for the sum total of drives activating the experienced individual" (*ibid.*, 104).

a man that it is little wonder Veblen mistook it for one. A business man, long since secure economically, works himself into ill-health, and sometimes even back into poverty, for the sake of carrying on his plans. What was once an instrumental technique becomes a master-motive.

Neither necessity nor reason can make one contented permanently on a lonely island or on an isolated farm after one is adapted to active, energetic city life. The acquired habits seem sufficient to urge one to a frenzied existence, even though reason and health demand the simpler life.

The pursuit of literature, the development of good taste in clothes, the use of cosmetics, the acquiring of an automobile, strolls in the public park, or a winter in Miami—all may first serve, let us say, the interests of sex. But every one of these instrumental activities may become an interest in itself, held for a life time, long after the erotic motive has been laid away in lavender. People often find that they have lost allegiance to their original aims because of their deliberate preference for the many ways of achieving them.

The maternal sentiment offers a final illustration. Many young mothers bear their children unwillingly, dismayed at the thought of the drudgery of the future. At first they may be indifferent to, or even hate, their offspring; the 'parental instinct' seems wholly lacking. The only motives that hold such a mother to child-tending may be fear of what her critical neighbors will say, fear of the law, a habit of doing any job well, or perhaps a dim hope that the child will provide security for her in her old age. However gross these motives, they are sufficient to hold her to her work, until through the practice of devotion her burden becomes a joy. As her love for the child develops, her earlier practical motives are forgotten. In later years not one of these original motives may operate. The child may be incompetent, criminal, a disgrace to her, and far from serving as a staff for her declining years, he may continue to drain her resources and vitality. The neighbors may criticize her for indulging the child, the law may exonerate her from allegiance; she certainly feels no pride in such a child; yet she sticks to him. The tenacity of the maternal sentiment under such adversity is proverbial.

Such examples from everyday experience could be multiplied *ad infinitum*. The evidence, however, appears in sharper outline when it is taken from experimental and clinical studies. In each of the following instances some new function emerges as an independently structured

unit from preceding functions. The activity of these new units does not depend upon the continued activity of the units from which they developed.

(1) *The circular reflex.* Everyone has observed the almost endless repetition of acts by a child. The good-natured parent who picks up a spoon repeatedly thrown down by a baby wearies of this occupation long before the infant does. Such repetitive behavior, found likewise in early vocalization (babbling), and in other early forms of play, is commonly ascribed to the mechanism of the circular reflex.[4] It is an elementary instance of functional autonomy; for any situation where the consummation of an act provides adequate stimulation for the repetition of the *same* act does not require any backward tracing of motives. The act is self-perpetuating until it is inhibited by new activities or fatigue.

(2) *Conative perseveration.* Many experiments show that incompleted tasks set up tensions that tend to keep the individual at work until they are resolved. No hypothesis of self-assertion, rivalry, or any other basic need, is required. The completion of the task itself has become a quasi-need with dynamic force of its own. It has been shown, for example, that interrupted tasks are better remembered than completed tasks,[5] that an individual interrupted in a task will, even in the face of considerable opposition return to that task,[6] that even trivial tasks undertaken in a casual way become almost haunting in character until they are completed.[7]

Conative perseveration of this order is stronger if an empty interval of time follows the period of work, showing that *left to itself*, without the inhibiting effect of other duties or activities, the motive grows stronger and stronger. The experiment of Kendig proves this point, as well as that of C. E. Smith.[8] The latter investigator demonstrated that there is more success in removing a conditioned fear if the de-conditioning process is commenced immediately. After a twenty-four hour delay the fear has become set, and is more difficult to eradicate. Hence the sound advice to drivers of automobiles or airplanes

[4] E. B. Holt, *Animal Drive and the Learning Process*, 1931, esp. Chaps. VII and VIII.

[5] B. Zeigarnik, Über das Behalten von erledigten und unerledigten Handlungen, *Psychol. Forsch.*, 9, 1927, 1–86.

[6] M. Ovsiankina, Die Wiederaufnahme unterbrochener Handlungen, *ibid.*, 11, 1928, 302–379.

[7] I. Kendig, Studies in perseveration, *J. Psychol.*, 3, 1936, 223–264.

[8] C. E. Smith, Change in the apparent resistance of the skin as a function of certain physiological and psychological factors, A thesis deposited in the Harvard College Library, 1934.

who have been involved in an accident, that they drive again immediately to conquer the shock of the accident, lest the fear become set into a permanent phobia. The rule seems to be that unless specifically inhibited all emotional shocks, given time to set, tend to take on a compulsive autonomous character.

(3) *Conditioned reflexes not requiring reënforcement.* The pure conditioned reflex readily dies out unless the secondary stimulus is occasionally reënforced by the primary stimulus. The dog does not continue to salivate whenever it hears a bell unless sometimes at least an edible offering accompanies the bell. But there are innumerable instances in human life where a single association, *never* reënforced, results in the establishment of a life-long dynamic system. An experience associated only once with a bereavement, an accident, or a battle, may become the center of a permanent phobia or complex, not in the least dependent on a recurrence of the original shock.

(4) *Counterparts in animal behavior.* Though the validity of a principle in human psychology never depends upon its having a counterpart in animal psychology, still it is of interest to find functional autonomy in the lower organisms. For example, rats, who will first learn a certain habit only under the incentive of some specific tension, as hunger, will, after learning, often perform the habit even when fed to repletion.[9]

Another experiment shows that rats trained to follow a long and difficult path, will for a time persist in using this path, even though a short easy path to the goal is offered and even after the easier path has been learned.[10] Among rats as among human beings, old and useless habits have considerable power in their own right.

Olson studied the persistence of artificially induced scratching habits in rats. Collodion applied to the ears of the animal set up removing and cleaning movements. Four days later the application was repeated. From that time on the animals showed significantly greater number of cleaning movements than control animals. A month after the beginning of the experiment when the ears of the rats as studied by the microscope showed no further trace of irritation, the number of movements was still very great. Whether the induced habit spasm was permanently retained the experimenter does not say.[11]

[9] J. D. Dodgson, Relative values of reward and punishment in habit formation, *Psychobiol.*, 1, 1917, 231–276. This work has already been interpreted by K. S. Lashley as favoring Woodworth's dynamic theory as opposed to Freud's (Contributions of Freudism to psychology: III. Physiological analysis of the libido, *Psychol. Rev.*, 31, 1924, 191–202).

[10] H. C. Gilhousen, Fixation of excess distance patterns in the white rat, *J. Comp. Psychol.*, 16, 1933, 1–23.

[11] W. C. Olson, *The Measurement of Nervous Habits in Normal Children*, 1929, 61–65.

(5) *Rhythm.* A rat whose activity bears a definite relation to his habits of feeding (being greatest just preceding a period of feeding and midway between two such periods) will, even when starved, display the same periodicity and activity. The acquired rhythm persists without dependence on the original periodic stimulation of feeding.[12]

Even a mollusc whose habits of burrowing in the sand and reappearing depend upon the movements of the tide, will, when removed from the beach to the laboratory, continue for several days in the same rhythm without the tide. Likewise certain animals, with nocturnal rhythms advantageous in avoiding enemies, obtaining food, or preventing excessive evaporation from the body, may exhibit such rhythms even when kept in a laboratory with constant conditions of illumination, humidity, and temperature.[13]

There are likewise instances where acquired rhythms in human life have taken on a dynamic character. Compulsive neurotics enter upon fugues or debauches, apparently not because of specific stimulation, but because "the time has come." A dipsomaniac, in confinement and deprived for months of his alcohol, describes the fierceness of the recurrent appetite (obviously acquired) as follows.

> Those craving paroxysms occur at regular intervals, three weeks apart, lasting for several days. They are not weak, nambypamby things for scoffers to laugh at. If not assuaged with liquor they become spells of physical and mental illness. My mouth drools saliva, my stomach and intestines seem cramped, and I become bilious, nauseated, and in a shaky nervous funk.[14]

In such states of drug addiction, as likewise in states of hunger, lust, fatigue, there is to be sure a physical craving, but the rhythms of the craving are partially acquired, and are always accentuated by the mental habits associated with it. For instance, eating in our civilized way of life takes place not because physical hunger naturally occurs three times a day, but because of habitual rhythms of expectancy. The habit of smoking is much more than a matter of craving for the specific narcotic effects of tobacco; it is a craving for the motor ritual and periodic distraction as well.

(6) *Neuroses.* Why are acquired tics, stammering, sexual perversions, phobias, and anxiety so stubborn and so often incurable? Even psychoanalysis, with its deepest of depth-probing, seldom succeeds in

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[12] C. P. Richter, A behavioristic study of the activity of the rat, *Comp. Psychol. Monog.*, 1, 1922, (no. 2), 1–55.

[13] S. C. Crawford, The habits and characteristics of nocturnal animals, *Quart. Rev. Biol.*, 9, 1934, 201–214.

[14] Inmate Ward Eight, *Beyond the Door of Delusion*, 1932, 281.

effecting *complete* cures in such cases, even though the patient may feel relieved or at least reconciled to his difficulties after treatment. The reason seems to be that what are usually called 'symptoms' are in reality something more. They have set themselves up in their own right as independent systems of motivation. Merely disclosing their roots does not change their independent activity.[15]

(7) *The relation between ability and interest.* Psychometric studies have shown that the relation between ability and interest is always positive, often markedly so. A person likes to do what he can do well. Over and over again it has been demonstrated that the skill learned for some external reason, turns into an interest, and is self-propelling, even though the original reason for pursuing it has been lost. A student who at first undertakes a field of study in college because it is prescribed, because it pleases his parents, or because it comes at a convenient hour, often ends by finding himself absorbed, perhaps for life, in the subject itself. He is not happy without it. The original motives are entirely lost. What was a means to an end has become an end in itself.

Furthermore, there is the case of genius. A skill takes possession of the man. No primitive motivation is needed to account for his persistent, absorbed activity. It just *is* the alpha and omega of life to him. It is impossible to think of Pasteur's concern for health, food, sleep, or family, as the root of his devotion to his work. For long periods of time he was oblivious of them all, losing himself in the white heat of research for which he had been trained and in which he had *acquired* a compelling and absorbing interest.

A much more modest instance is the finding of industrial research that when special incentives are offered and work speeded up as a consequence, and then these special incentives removed, *the work continues at the speeded rate.* The habit of working at a faster tempo persists without external support.

(8) *Sentiments vs. instincts.* Every time an alleged instinct can by rigid analysis be demonstrated not to be innate but acquired, there is in this demonstration evidence for functional autonomy. It is true enough that maternal conduct, gregariousness, curiosity, workman-

[15] The case of W. E. Leonard, *The Locomotive God*, 1927, is instructive in this regard. An intense phobia was not relieved by tracing its history backward to the start of life. Even though he could explain why he was once frightened for a very good reason (by a locomotive), the author is quite unable to explain why now he is frightened *for no particular reason.* Such neuroses, and psychotic delusional systems as well, often acquire a "strangle hold," and the task of dislodging them is usually more than therapeutic skill is equal to.

ship, and the like, have the tenacity and compelling power that instincts are supposed to have. If they are not instincts, then they must be autonomous sentiments with as much dynamic character as has been attributed to instincts. It is not necessary here to review all the arguments in favor of regarding such alleged instincts as acquired sentiments.

(9) *The dynamic character of personal values.* When an interest-system has once been formed it not only creates a tensional condition that may be readily aroused, leading to overt conduct in some way satisfying to the interest, but it also acts as a silent agent for selecting and directing any behavior related to it. Take the case of people with strongly marked esthetic interests. Experiments with the word-association test have shown that such people respond more quickly to stimulus-words connected with this interest than to words relating to interests they lack.[16] Likewise, in scanning a newspaper they will observe and remember more items pertaining to art; they also take a greater interest in clothes than do non-esthetic people; and when they are asked to rate the virtues of others, they place esthetic qualities high. In short the existence of a well-established acquired interest exerts a directive and determining effect on conduct just as is to be expected of any dynamic system. The evidence can be duplicated for many interests other than the esthetic.[17]

Critique of Functional Autonomy

Objections to the principle of autonomy may be expected from two sides. Behaviorists will continue to prefer their conception of organic drive with its capacity for manifold conditioning by ever receding stimuli. Whereas purposivists will be unwilling to accept a pluralistic principle that seems to leave motives so largely at the mercy of learning.

The behaviorist is well satisfied with motivation in terms of organic drive and conditioning because he feels that he somehow has secure anchorage in physiological structure (The closer he approaches physiological structure the happier the behaviorist is.) But the truth of the matter is that the neural physiology of organic drive and conditioning is no better established, and no easier to imagine, than is the neural physiology of the type of complex autonomous units of motivation here described.

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[16] H. Cantril, General and specific attitudes, *Psychol. Monog.*, 42, 1932, (no. 192), 1–109.
[17] H. Cantril and G. W. Allport, Recent applications of the *study of values*, *J. Abnorm. & Soc. Psychol.*, 28, 1933, 259–273.

Two behavioristic principles will be said to account adequately for the instances of functional autonomy previously cited, viz., the circular reflex and cross-conditioning. The former concept, acceptable enough when applied to infant behavior, merely says that the more activity a muscle engages in, the more activity of the same sort does it engender through a self-sustaining circuit.[18] This is, to be sure, a clear instance of autonomy, albeit on a primitive level, oversimplified so far as adult conduct is concerned. The doctrine of cross-conditioning refers to subtle recession of stimuli, and to the intricate possibility of cross-connections in conditioning. For instance, such ubiquitous external stimuli as humidity, daylight, gravitation, may feed collaterally into open channels of activity, arousing mysteriously and unexpectedly a form of conduct to which they have unconsciously been conditioned. For example, the angler whose fishing expeditions have been accompanied by sun, wind, or a balmy June day, may feel a desire to go fishing whenever the barometer, the thermometer, or the calendar in his city home tells him that these conditions prevail.[19] Innumerable such crossed stimuli are said to account for the arousal of earlier patterns of activity.

Such a theory inherits, first of all, the well-known difficulties resident in the principle of conditioning whenever it is made the sole explanation of human behavior. Further, though the reflex circle and cross-conditioning may in fact exist, they are really rather trivial principles. They leave the formation of interest and its occasional arousal almost entirely to chance factors of stimulation. They give no picture at all of the spontaneous and variable aspects of traits, interests, or sentiments. These dispositions are regarded as purely *reactive* in nature; the stimulus is all-important. The truth is that dispositions *sort out* stimuli congenial to them, and this activity does not in the least resemble the rigidity of reflex response.[20]

A variant on the doctrine of cross-conditioning is the principle of redintegration.[21] This concept admits the existence of highly integrated dispositions of a neuropsychic order. These dispositions can be aroused *as a whole* by any stimulus previously associated with their

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[18] E. B. Holt, *op. cit.*, 38.

[19] *Ibid.*, 224.

[20] The basic fact that complex "higher" centers have the power of inhibiting, selecting, and initiating the activity of simpler segmental responses is a fact too well established to need elaboration here. It constitutes the very foundation of the psychophysiological theories advanced by Sherrington, Herrick, Dodge, Köhler, Troland, and many others.

[21] Cf. H. L. Hollingworth, *Psychology of the Functional Neuroses*, 1910.

functioning. In this theory likewise, the disposition is regarded as a rather passive affair, waiting for reactivation by some portion of the original stimulus. Here again the variability of the disposition and its urge-like quality are not accounted for. The stimulus is thought merely to reinstate a complex determining tendency. Nothing is said about how the stimuli themselves are *selected*, why a motive once aroused becomes insistent, surmounting obstacles, skillfully subordinating conflicting impulses, and inhibiting irrelevant trains of thought.

In certain respects the principle of autonomy stands midway between the behavioristic view and the thoroughgoing purposive psychology of the hormic order. It agrees with the former in emphasizing the acquisition of motives, in avoiding an a priori and unchanging set of original urges, and in recognizing (as limited principles) the operation of the circular response and cross-conditioning. It agrees with the hormic psychologist, however, in finding that striving-from-within is a far more essential characteristic of motive than stimulation-from-without. It agrees likewise in distrusting the emphasis upon stomach contractions and other "excess and deficit stimuli" as "causes" of mature behavior. Such segmental sources of energy even when conditioned cannot possibly account for the "go" of conduct. But functional autonomy does not rely as does hormic theory upon modified instinct, which after all is as archaic a principle as the conditioning of autonomic segmental tensions, but upon the capacity of human beings to replenish their energy through a plurality of constantly changing systems of a dynamic order.

The hormic psychologist, however, will not accept the autonomy of new motivational systems. If mechanisms can turn into drives, he asks, why is it that habits and skills as they become exercised to the point of perfection do not acquire an ever increasing driving force?[22] The mechanisms of walking, speaking, or dressing, cannot be said to furnish their own motive-power. One walks, speaks, or dresses in order to satisfy a motive entirely external to these learned skills.[23]

The criticism is sufficiently cogent to call into question Woodworth's form of stating the principle, viz., "mechanisms may become drives." It is not an adequate statement of the case.

Looking at the issue more closely it seems to be neither the perfected

[22] W. McDougall, Motives in the light of recent discussion, *Mind*, 29, 1920, 277–293.

[23] Though this objection is usually valid, it is not always so, for there are cases where the liking for walks, for talking for the sake of talking, or for dressing, playing games, etc., seems to be a self-sustaining motivational system.

talent nor the automatic habit that has driving power, but the imperfect talent and the habit-in-the-making. The child who is *just learning* to speak, to walk, or to dress is, in fact, likely to engage in these activities for their own sake, precisely as does the adult who has an *unfinished* task in hand. He remembers it, returns to it, and suffers a feeling of frustration if he is prevented from engaging in it. Motives are always a kind of striving for some form of completion; they are unresolved tension, and demand a "closure" to activity under way. (Latent motives are dispositions that are easily thrown by a stimulus or by a train of associations into this state of active tension.) The active motive subsides when its goal is reached, or in the case of a motor skill, when it has become at last automatic. The novice in automobile driving has an unquestionable impulse to master the skill. Once acquired the ability sinks to the level of an *instrumental* disposition and is aroused only in the service of some other *driving* (unfulfilled) motive.

Now, in the case of the permanent interests of personality, the situation is the same. A man whose motive is to acquire learning, or to perfect his craft, can never be satisfied that he has reached the end of his quest, for his problems are never completely solved, his skill is never perfect. Lasting interests are recurrent sources of discontent, and from their incompleteness they derive their forward impetus. Art, science, religion, love, are never perfected. Motor skills, however, are often perfected, and beyond that stage they seldom provide their own motive power. It is, then, only mechanisms-on-the-make (in process of perfecting) that serve as drives. With this emendation, Woodworth's view is corrected, and McDougall's objection is met.[24]

Implications of Functional Autonomy

The principle of functional autonomy accounts, as no other principle of dynamic psychology is able to do, for the concrete impulses that lie at the root of personal behavior. It is thus the first step in establishing a basis for the more realistic study of unique and individual forms for personality. "But how—" the traditionalists may cry, "how are we ever to have a *science* of unique events? Science must generalize." So it must, but it is a manifest error to assume that a general principle of motivation must involve the postulation of abstract or general motives. What the objectors forget is that *a general law may be a law that tells*

[24] This theory embraces very easily the work of K. Lewin and his associates upon the nature of "quasi-needs." The urgency of these needs is greatest just before a goal is reached, after which time the motive subsides completely.

how uniqueness comes about. The principle of functional autonomy is general enough to meet the needs of science, but particularized enough in its operation to account for the uniqueness of personal conduct. Its specific advantages stand out in the following summary.

(*1*) It clears the way for a completely dynamic psychology of *traits*, *attitudes*, *interests*, and *sentiments*, which can now be regarded as the ultimate and true dispositions of the mature personality.

(*2*) It avoids the absurdity of regarding the energy of life now, in the *present*, as somehow consisting of early archaic forms (instincts, prepotent reflexes, or the never-changing Id). Learning brings new systems of interests into existence just as it does new abilities and skills. At each stage of development these interests are always contemporary; whatever drives, drives *now*.

(*3*) It dethrones the stimulus. A motive is no longer regarded as a mechanical reflex or as a matter of redintegration, depending entirely upon the capricious operation of a conditioned stimulus. In a very real sense dispositions *select* the stimuli to which they respond, even though *some* stimulus is required for their arousal.

(*4*) It readily admits the validity of all other established principles of growth. Functional autonomy recognizes the products of differentiation, integration, maturation, exercise, imitation, suggestion, conditioning, trauma, and all other processes of development; and allows, as they do not, considered by themselves, for the preservation of these products in significant motivational patterns.

(*5*) It places in proper perspective the problems of the origin of conduct by removing the fetish of the genetic method. Not that the historical view of behavior is unimportant for a complete understanding of personality, but so far as *motives* are concerned the cross-sectional dynamic analysis is more significant. Motives being always contemporary should be studied in their present structure. Failure to do so is probably the chief reason why psychoanalysis meets so many defeats, as do all other therapeutic schemes relying too exclusively upon uncovering the motives of early childhood.

(*6*) It accounts for the force of delusions, shell-shock, phobias, and all manner of compulsive and maladaptive behavior. One would expect such unrealistic modes of adjustment to be given up as they are shown to be poor ways of confronting the environment. Insight and the law of effect should both remove them—but too often they have acquired a strangle hold in their own right.

(7) At last we can account adequately for socialized and civilized behavior. The principle supplies the correction necessary to the faulty logic of *bellum omnium contra omnes*. Starting life, as a completely selfish being, the child would indeed remain entirely wolfish and piggish throughout his days unless genuine transformations of motives took place. Motives being completely alterable, the dogma of Egoism turns out to be a callow and superficial philosophy of behavior, or else a useless redundancy.

(8) It explains likewise why a person often *becomes* what at first he merely *pretends* to be—the smiling professional hostess who grows fond of her once irksome rôle and is unhappy when deprived of it; the man who for so long has counterfeited the appearance of self-confidence and optimism that he is always driven to assume it; the prisoner who comes to love his shackles. Such *personae*, as Jung observes, are often transformed into the real self. The mask becomes the *anima*.

(9) The drive behind genius is explained. Gifted people demand the exercise of their talents, even when no other reward lies ahead. In lesser degree the various hobbies, the artistic, or the intellectual interests of any person show the same significant autonomy.

(10) In brief, the principle of functional autonomy is a declaration of independence for the psychology of personality. Though in itself a general law, at the same time it helps to account, not for the abstract motivation of an impersonal and therefore non-existent mind-in-general, but for the concrete, viable motives of each and every mind-in-particular.

VERANTWORTUNG

By Otto Klemm, Universität Leipzig
Editor, *Zeitschrift für angewandte Psychologie und Charakterkunde*
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In der Lehre vom Willen pflegt die Entwickelung geschildert zu werden, die von den Urformen eines willkürlichen Tuns hinaufführt zu den ausgegliederten Einzelakten des hell bewusst gewordenen Wollens. Wie auch immer im einzelnen dieses Drängen und Streben, dieses Sehnen und Begehren sich entfalten möge: eine Stelle gibt sich so deutlich als eine Wende an dieser täterischen Seite unseres Seelenlebens zu erkennen, dass sie von niemandem übersehen zu werden vermag: der Übergang von den Trieben zu denjenigen Arten eines willkürlichen Tuns, die wir als das eigentliche Wollen bezeichnen.

Das entscheidende Kennzeichen hierfür aber bleibt es, dass der Mensch für diese seine Willenshandlungen verantwortlich ist. Es darf als ein unerschütterbarer Befund in Anspruch genommen werden, dass es innerhalb unseres willkürlichen Tuns einen solchen Kernbereich gibt, der mit Verantwortlichkeit ausgerüstet ist: und zu diesem gehört zugleich das Wollen im engeren Sinne oder das eigentliche Wollen.

Was ich eben aussprach, ist jederzeit im eigenen Erleben nachprüfbar: es verlöre der Aufbau unseres Willenslebens seinen Halt, gäbe es nicht auch gegenüber noch so überschäumenden Trieben jene verantwortliche Entschliessung ihres Trägers, an der sie wie an einem unüberflutbaren Felsen sich brechen können. Indem wir aber nun über den Einzelnen hinaus auf die Gemeinschaft blicken, wird ein folgenreicher Zusammenhang sichtbar.

Gemeinsames Tun übt weithin bindende Kräfte aus: eingefügt in die gewachsenen Ordnungen der Gemeinschaft vollzieht sich das Einzelleben als das eines Gliedes im Ganzen. Überlieferte Formen des Handelns übertragen sich auf den Einzelnen, ohne von ihm eine Wahl oder im besonderen eine freie Entscheidung zu erwarten, geschweige denn zu verlangen. Je fester solche bindenden Kräfte von Sitte und Glauben, von Familie und öffentlicher Ordnung sind, umso vollständiger zugleich bleibt das seelische Geschehen in ihnen eingebettet.

Aber im Vollzuge jener Durchgliederung, die nun einmal aller

seelischen Entwickelung innewohnt, vermag sich das Einzeltun aus solchen Bindungen zu lösen. Das bislang triebartig vom Ganzen getragene Tun wird zu einem ausgegliederten Einzeltun. In demselben Masse aber lädt es sich mit Verantwortlichkeit. Innerhalb unseres eigenen voll entwickelten und durchgegliederten Willenslebens sind uns solche Akte eines Einzelwollens, bis hinein in die schlichtesten Verrichtungen des Alltags, so geläufig und so selbstverständlich geworden, dass der angedeutete Zusammenhang mit der Gemeinschaft uns oft ganz entschwindet. Er wird aber sofort wieder lebendig, sobald dieses Einzeltun in ein gemeinschaftliches sich einfügt. Wer sich freiwillig unter eine Norm gemeinschaftlichen Handelns stellt, kehrt zu jenen ursprünglichen Formen des gemeinschaftlichen Tuns, auf höherer Stufe, zurück und geht wieder als Glied im Ganzen auf. Wer aber dabei die Verantwortung übernimmt, der lebt zugleich mit seinem eigenen Wollen in allen denen, die ihm folgen. Hingabe und Verantwortung hängen hier aufs innigste zusammen.

Erst als Träger eines solchen verantwortlichen Tuns wird der Mensch zu einer Person, die den blossen Triebhandlungen übergeordnete Forderungen entgegenzusetzen vermag. Person sein kann der Mensch aber wiederum nur als Glied einer Gemeinschaft. Verantwortliches Wollen reicht notwendig über das Einzeldasein hinaus: denn alle Verantwortung ist Verantwortung vor einem überindividuellen Bereiche. Verantwortung zu tragen hat überhaupt nur so lange einen Sinn, als innerhalb einer Gemeinschaft eines ihrer Glieder zu einem selbständigen Handeln berufen ist. Solche Gemeinschaftsbereiche sind ausgebreitet in der lebendig flutenden Gegenwart des Volksganzen. Sie erstrecken sich zeitlich in das Kommen und Gehen der Geschlechter und erfüllen die Abhängigkeit vom Früheren mit der Verantwortung vor dem Künftigen. Sie tragen hinaus, gleichsam über Raum und Zeit, bis zur Verbindung des Einzelnen mit dem Grunde aller Dinge—hin bis zur einsamsten Zwiesprache mit Gott, die zugleich höchste Gemeinschaft ist.

An seine Triebe bleibt der Mensch allezeit gebunden, für sie ist er letzthin nicht verantwortlich—aber es ist ihm gegeben, sich die überindividuellen Forderungen zu eigen zu machen: und indem er dies tut, wird er zur Person. Die Freiheit der Person liegt allezeit darin beschlossen, den Trieben das eigentliche Wollen überordnen zu können. In nichts anderem kann sie begründet sein, als darin, dass in den Wirkungszusammenhang der Triebe, die mich in Anspruch nehmen, steuernd eine Entschiedung einzugreifen vermag, die nicht mehr aus dem

Sitz der Triebe fliesst, sondern eben aus einem anderen Bezirke. Nur indem der Mensch, der zum selbständigen Handeln erwachte, in sich selbst hineinnimmt, was zu der Gemeinschaft gehört, vermag er auch über sich selbst hinaus zu wachsen. Kants kategorischer Imperativ brachte dies in erhabener Strenge zum Ausdruck.

Solche Freiheit des Willens hat garnichts zu tun mit der Art und Weise, wie im einzelnen aus einer nachweisbaren Verbindung von Motiven und sonstigen Antrieben ein Entschluss tatsächlich zustande kommen mag. Ob nun im Sinne einer streng kausalen Denkweise alle Ereignisse, also auch unsere Entschlüsse und unser Tun, vorausbestimmt sind, oder ob es neben einem abgrenzbaren Bereiche der kausal bestimmten auch zufällige und schliesslich völlig "freie" gebe—unabhängig, ja geradezu unberührbar von einer hier je zu erreichenden theoretischen Einsicht, bleibt doch das Bewusstsein der eigenen Verantwortlichkeit. Eine begangene Schuld wird durch den Indeterminismus nicht schwerer und durch den Determinismus nicht leichter: sie kann es nicht werden, da sie aus einer anderen Dimension herrührt, als die Festigkeit oder die Lockerheit in dem ursächlichen Zusammenhang einzelner Ereignisse: sie wurzelt darin, dass der Träger dieses Tuns Glied einer Gemeinschaft ist, und dieser Zusammenhang kann durch die Wirkungsschicht jener einzelnen Antriebe weder geschützt noch angetastet werden. Darum sagte Epiktet mit Recht: "Einen Räuber der Willensfreiheit gibt es nicht." Nein, es gibt ihn wirklich nicht. Denn der Person des Menschen wird nichts geraubt und nichts geschenkt. Wir haben bisweilen Grund dazu, dem Menschen die Verantwortung für sein Tun abzusprechen: in demselben Masse aber, in dem dies zutrifft, weicht zugleich die "Person" aus dem Geschehen zurück, in das er verstrickt bleibt. Darum bleibt es gewiss für unzählige praktische Entscheidungen in Kraft, dass der Mensch diesmal frei und ein andermal nicht frei gehandelt habe; der scheinbare Widerspruch löst sich leicht dahin auf, dass eben der eigentliche Träger dieser beiden Handlungen nicht der gleiche ist.

Wenn diese Gedanken richtig sind, dann entspringt die Verantwortlichkeit aus der Bedeutung unseres Tuns für die Gemeinschaft. Das muss allerdings recht verstanden werden. Die Mutter schützt ihr Kind triebhaft und machtvoll können hieraus Willenschandlungen von reinster Verantwortlichkeit entspringen. Wer mit reichen Gaben geistiger Art ausgerüstet ist, kann die Verantwortung in sich spüren, sie zu pflegen und dieses Rüstzeug auch anzuwenden: wenn er auch in noch so grosser äusserer Vereinzelung dieser seiner Aufgabe sich hin-

gibt, so tut er es doch zugleich mit für andere und bleibt dadurch mit ihnen verbunden. Es kann auch eine Handlung, die die Pflicht gegen das Ganze erfüllt, zu den selbstischen Trieben in härtester Spannung stehen und ihnen in einem heldischen inneren Kampfe erst abgerungen sein: gemeinsam ist diesen und zahlreichen anderen Verantwortungsbereichen eben, dass in ihnen allen der Mensch als Glied eines Ganzen sich entfaltet.

Gegenüber diesem Bereiche seelischen Geschehens erhebt sich nun wie überall die Frage nach seinen strukturellen Grundlagen. Das Sprachgefühl leitet uns hier grosser Sicherheit auf den Charakter hin. In dem Charakter eines Menschen gründet sein Tun und Lassen, soweit er dafür eben verantwortlich ist, und für das er auch, solange er selbst im Lote ist, sich verantwortlich fühlt. Der Charakter eines Menschen in diesem innersten Sinne zeigt sich, sobald er als Glied einer Gemeinschaft und seine Geöffnetheit für die darin enthaltenen überindividuellen Werte auf die Probe gestellt wird.

Die strukturellen Grundlagen des seelischen Geschehens sind ja unter verschiedenen Blickrichtungen sichtbar geworden. Wer von dem Vollzuge einzelner begrenzter Leistungen ausgeht, hat das reich gegliederte Feld der Begabungen vor sich. In der Begabungsforschung wird dieses eigentliche Rüstzeug des Menschen durchgesehen. Den Unterschieden der intellektuellen Spannweite treten einzelne Fertigkeiten zur Seite—dieser kann singen und jener kann zeichnen, diesem eignet die geschickte Arbeitshand des Handwerkers und jenem das ruhige Auge des Schützen, diesem die Gabe feuriger Rede und jenem das wohlgeformte Gleichmass freundlicher Verse: kurzum, die Talente sind es, die hier auf farbiger Palette sich ausbreiten. Mögen sie auch inhaltlich noch so verschieden sein, das eine haben sie doch miteinander gemein, dass sie eben dazu taugen, um einzelne Aufgaben, die an den Menschen herantreten, zu bemeistern. Der Talentierte kann etwas—oft von vornherein—das der andere nicht kann. Talent zeigt sich in der Lösung einer Aufgabe. Der Talentierte hat vor anderen einen Vorsprung fast ähnlicher Art wie jener, der ein schärferes Werkzeug sein eigen nennt.

Von diesem Bereiche der eigentlichen Anlage unterscheidet sich nun deutlich genug jene andere Schicht, in welcher die dauernden Eigenarten des Gefühls- und Willenslebens wurzeln. Hier sprechen wir mit Vorliebe von dem Temperament oder auch von dem Naturell eines Menschen, spüren es aber zugleich, dass damit nur gleichsam die unmittelbar sichtbaren Züge getroffen sind. Schon das Sprachgefühl

warnt uns davor, den aus dem Leistungsbereiche stammenden Begriff der Anlage und der Begabung unbesehen auf die strukturellen Grundlagen für jene andere Seite des seelischen Geschehens zu übertragen. Es ist zwar auch von Gefühlsbeanlagungen und Willensbeanlagungen gesprochen worden: aber das klappert und klingt hölzern. Dem Menschen, der arm an Gemüt ist, fehlt nicht ein Talent—dieser Mangel greift viel tiefer. Die Tiefe, bis zu der ein Mensch durch ein Gefühl erschüttert zu werden vermag, ist nicht abhängig zu denken von irgend einer "Begabung," nein—das wäre ein hohles Gerede: sie ist in einem viel tieferen Sinne etwas, das ihm selbst zukommt, als seine Art da zu sein. Alles, was auf einer Beanlagung beruht, kann durch Übung gesteigert werden—vieles, zu dem uns die Begabung den Zugang verschafft, wird im eigentlichen Sinne erst erlernt. Ja, wir alle haben einmal gelebt, wir waren da, waren richtig einmal am Leben, ohne zu denken—aber auch ohne zu fühlen? Nein, das brauchte nicht gelernt zu werden. Sicherlich gibt es auch eine echte Veredelung und Vertiefung des Fühlens im Laufe der Entwicklung—aber dies ist kein Erlernen, sondern ein Wachsen; das Bild von dem Blühen und der reifenden Frucht stellt sich ein, und es ist mehr als nur ein Bild!

In welcher Weise nun solche Schwingungen des Zumuteseins ablaufen, und welche Wallungen von Trieben und täterischem Drange sich damit paaren, das wird sicherlich mit dem Begriffe des Temperaments zunächst auch nur teilweise umschlossen. Die historisch überlieferte Vierteilung der Temperamente legte hier einen Schematismus nahe, der sicherlich weit davon entfernt blieb, die Grundformen, auf die jenes wechselnde Farbenspiel zurüchführbar ist, auch nur annähernd zu umreissen: Immerhin genügt es in diesem Zusammenhange einzusehen, dass diese innerseelischen Bereiche in einer Strukturschicht gründen, die mit dem Namen Temperament von jener der Talente hinreichend deutlich unterschieden ist.

Stehen wir hier nun schon beim Charakter? Ja und Nein! Ja—insofern Reichtum oder Armut an Gefühlen im Vergleiche mit den Talenten gleichermassen mehr dem eigentliche Kerne der Persönlichkeit sich nähert, wie etwa die Stärke der Antriebskraft überhaupt, die einem Menschen innewohnt, oder ihre Schwäche. Und doch auch Nein—insofern als etwas im Menschen übrig bleibt, an das keine Schwingung eines Temperaments heranreicht. Denn was immer an Unterschieden zwischen den Talenten auf der einen und den Temperamenten auf der anderen Seite sich als aufzeigbar erweist, gemeinsam bleibt ihnen beiden doch, dass der Mensch sich dafür nicht verantwortlich fühlt.

Art und Umfang der Talente fallen dem Menschen zu: sie sind da oder sie fehlen; neben fruchtbarem Boden für geistiges Reifen liegt steiniger: sie zu haben ist Gunst und Glück, vielleicht auch Gnade, nicht aber Verdienst; sie nicht zu haben, ist Mangel und Kargheit, vielleicht auch Elend, nicht aber Schuld. Gleiches gilt für viele Unterschiede des Temperamentes. Welche Stimmungen ein Mensch hat in irgend einer Verwicklung des Lebens, dafür kann er nichts. Ob seine Gefühle leicht beweglich sind oder verhalten bleiben, das ist ihm nicht anzurechnen. Welchen Spielraum sein Naturell dem Drängen der Triebe lässt, das ist ein Befund gleich manchen anderen. Frische oder Mattheit, pralles Erfülltsein mit Lebensenergie oder Versickern in dünnes Rinnsal—solche und tausenderlei andere Farben unseres Lebensgefühls mögen bald den äusseren Bedingungen zugeordnet sein, bald auch sich als dauerndere Bestimmtheiten unseres inneren Seins erweisen: immer aber bleiben sie mit ihren wesentlichen Zügen der Sphäre der eigenen Verantwortlichkeit entrückt

Dies gilt auch weithin für diejenigen Strukturbereiche, die als des menschlichen Seins Grundformen unter den Begriff des Typus gefasst zuwerden pflegen. Indem die Typologie einen Menschen an eine bestimmte Stelle ihres Typensystems hinbringt, spricht sie ihm zugleich in begründeter Weise wohl geordnete Gruppen von Eigenschaften zu —sie zeigt ihn uns weltoffen oder verhalten, in Querschnitt- oder Längsschnitteinheit—und oft fällt es einem wie Schuppen von den Augen, wenn die erleuchtende Kraft solcher Grundgedanken eine schimmernde Fülle von Einzelheiten eben zu einem Gesamtzuge am Menschen erhebt. Immer aber ist es doch so: irgend einen Typ hat jeder Mensch. Welchen? Dafür kann er nichts. Er ist ihm in der Grundzügen sicherlich angeboren; zu dieser seiner typischen Lebensform erwacht der Mensch, und in ihr kommt er zu sich selbst.

Dies alles ist in Ordnung und doch liegt dahinter noch eine innerste Schicht menschlichen Seins: eben der eigentliche, persönliche Kern, der Charakter. Bei völliger Gleichheit des Typus kann es eben doch noch Unterschiede der Menschen geben, die zutiefst in ihrer seelischen Substanz wurzeln. Nicht an seinem Typ verzweifelt der Zusammenbrechende, sondern an sich selbst, d.h. an seiner Substanz. Dem Menschen wohnt etwas inne, das eben er selber in einer unüberbietbaren Weise ist: und diese Stelle in ihm ist zugleich der Hort der Verantwortlichkeit. Durch sie hängt das abgegrenzte Einzelleben zusammen mit dem Ganzen, in ihr trägt das Vergängliche zugleich den fernen Glanz des Unvergänglichen: sie ist es, die eigentlich den Menschen

ausmacht. Oder stellt sich etwa nicht mit Notwendigkeit die Überzeugung ein: was den Menschen wirklich angeht, wovon er am tiefsten betroffen ist, was ihm teuer ist, woran sein Herz mit allen Fasern hängt, das zeigt mir, was ihn im Innersten ausmacht.

Solcher Hingabe überhaupt fähig zu sein, ist ein menschentümlicher Zug im tiefsten Sinne: er prägt den Menschen zum Gliede eines ihn tragenden Ganzen, eben einer Gemeinschaft. Hingabe an eine Sache—mag diese auch noch so sehr in ihrem unmittelbar sichtbaren Sein aus allen menschlichen Beziehungen herauslösbar sein—ist doch immer eine Geöffnetheit für überindividuelle Bereiche, die in das eigene begrenzte Eigenleben hineinzunehmen zugleich eine Verbindung mit der Gemeinschaft bedeutet. Hingabe an eine Sache oder an eine Aufgabe ist nur so lange echte Hingabe, als sie auch zum Tun bereit macht. Nur der Mensch ist wirklich hingegeben, der solche Forderungen zu seinen eigenen macht—zu seinen eigenen, sage ich, und damit wiederum übernimmt er Verantwortung und tritt eben selbst mit diesem seinem innersten Kern in den Kreis des Geschehens ein.

Man deute dieses nicht falsch. Keineswegs soll in Abrede gestellt werden, dass die verständnisvolle Pflege einer Begabung zu einem Leistungsgewinne hinführt, und dass es ebenso eine echte Veredelung des Gefühlslebens und der Welt der Triebe gibt. Kein Zweifel kann daran bestehen, dass der Umgang mit dem Edlen in diese der Verantwortlichkeit des Menschen entzogenen Bereiche hineinstrahlt und in ihnen bleibende Wirkung zurücklässt. Und in demselben Masse nun, als der Mensch sich ehrlich und mit Stetigkeit hierum bemüht, wird er stolz auf sein Können, so weit es seine Talente angeht, und spürt nun auch mit Genugtuung, wie die Schwingungen seines Temperaments immer fügsamer sich anschmiegen an das, was er selbst für sich zur Geltung erhoben hat. Wer es verabsäumt, hat auf der Zunge die schmerzliche Bitternis des Gedankens, was hätte ich alles daraus machen können! Vertane Stunden klopfen mahnend an! In diesem Sinne rücken dann also Talente und Temperament doch wieder in den Bereich der Verantwortlichkeit: und zwar genau so weit, als sie von jener Kraft des Menschen ergriffen worden sind, die in seinem Charakter—jetzt diesen Begriff im engsten Sinne genommen—wurzelt. Tritt nun aber der "Pflege der Begabung" nicht eine echte "Bildung des Charakters" zur Seite? Ja—so ist es. Nur eben zielt diese letzthin nicht mehr auf Einzelzüge am Menschen: den Charakter bilden heisst, den Menschen zum Glied einer Gemeinschaft machen; heisst zugleich, ihm und seinem Leben diesen Sinn geben und für immer zu eigen machen.

Der Charakter tritt daher in den Vollzug des Lebens niemals als ein blosses Ereignis hinein: jederzeit stellt sich zu ihm eine Beziehung her, die das Sollen und das Nichtsollen kernhaft und wesentlich in sich schliesst. Es bedarf nicht vieler Worte, um dieses auszumachen. Sich in dem Charakter eines Menschen zu täuschen, ist eine Bitternis völlig anderer Art als zu bemerken, dass er für Erlernung einer fremden Sprache nicht begabt ist. Der Mensch ist überhaupt nicht mit einem guten oder einem schlechten Charakter begabt, so wie er eben der Träger einzelner oder vieler Talente ist. Vielmehr diese Stärke oder Schwäche, die Treue oder Arglist, diese Echtheit oder dieser blosse Schein, das ist er eben selbst. Und mögen wir uns noch so deutlich ins Bewusstsein heben, dass ein Verbrechercharakter in einer Erbanlage vorgeformt ist: alle solche Einsicht in die Bedingungen, denen sein Einzeldasein zuzuschreiben ist, sie mindert nicht im geringsten die Selbstverständlichkeit, mit der wir dieses Nichtseinsollende ablehnen, ja es verabscheuen, und vor allem aber die Verantwortlichkeit trotzdem in ihrer vollen Schwere bestehen lassen.

Sie muss bestehen bleiben, da sie jederzeit und in unlösbarer Weise eine Verantwortung vor der Gemeinschaft ist. Lösen wir einen Menschen gedanklich aus der Gemeinschaft heraus, so spüren wir es: ein solcher Robinson kann zwar immer noch geschickt oder ungeschickt handeln, klug oder unklug, aber mit zunehmendem Grade der Vereinzelung verlören wohl die Fragen nach der Verantwortung ihren Sinn. Nie könnte er eine Schuld auf sich laden. Das gleiche gälte auch noch für eine blosse Summe oder einen Haufen von Einzelwesen. Erst sobald das Tun des Menschen zu einem Tun innerhalb einer Gemeinschaft wird, gewinnt jener innerste Bezirk seiner Persönlichkeit einen Sinn. Verantwortung entzündet sich überall dort, wo der Mensch als Einzelwesen und zugleich als Glied des Ganzen handelt: sein Charakter ist dasjenige an ihm, was jene Möglichkeit willkürlichen Tuns der Gliedschaft am Ganzen unterstellt.

Glied zu sein eines Ganzen—das kann sich in den verschiedenartigsten Bezirken des Lebendigen auswirken; von der schlichten und innigen Sorge der Mutter für das Kind, bis hinauf zu der Verantwortung vor dem überirdischen. Auch wer im grüblerischen Denken oder in religiöser Ergriffenheit um den Zusammenhang mit dem letzten Grunde aller Dinge ringt, fühlt eine Verantwortung vor dem höchsten, die zu tragen ihm aufgegeben ist.

Glied zu sein eines Ganzes—dies kann auch niemals gefasst werden etwa als ein besonderer Zug am Erleben, der neben anderen stünde—es

bedeutet vielmehr, dass ein wirklich von innen heraus verbundenes Tun—es braucht nicht immer auch gleichgerichtet zu sein—den Träger dieses Tuns mit einer Gemeinschaft zusammenschliesst.

Glied zu sein eines Ganzen—das mag oft in eine leidvolle Spannung treten zu den Trieben, die im Einzelmenschen beschlossen sind: diese zu meistern eben durch Stärke des Charakters bleibt des Menschen höchstes Ziel. Hineinzureichen—durch den Willen—in die Gemeinschaft weitet die Enge des Einzeldaseins—hebt empor zu dem nicht Vergänglichen—nähert die Menschen.

DIE PROBLEME DER CHARAKTERKUNDE

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In der wissenschaftlichen Erforschung der seelischen Wirklichkeit hat sich während der letzten Jahrzehnte—vorallem in Deutschland—eine Entwicklung vollzogen, die man als "Wendung zum Charakter" bezeichnen kann.[1] Sie geschah zweifellos aus einem gesteigerten Bedürfnis, die Seelenkunde dem Leben dienstbar zu machen. In ihrer Wurzel ist diese Wandlung eine solche der Fragestellung, die notwendigerweise auch eine gewisse Wandlung der Methode zur Folge hat.

I. Allgemein-Psychologische und Charakterologische Fragestellung

Es wäre durchaus falsch, wollte man die genannte Entwicklung lediglich als eine Abkehr von den bisherigen Problemen und Methoden einer "Allgemeinen Psychologie" verstehen. Letzten Endes handelt es sich nicht um eine Abkehr vom Alten, sondern um eine Ergänzung und Ausweitung der bisherigen Fragestellung. Die "Allgemeine Psychologie" betrachtet und untersucht das Seelenleben unter einer möglichst weitgehenden methodischen und begrifflichen Herauslösung unterscheidbarer Grundformen von Vorgängen und Zuständen noch abgesehen von den besonderen complexen Ausprägungen dieser Formen im einzelnen Menschen. Sie spricht von Gedächtnis oder von den intellektuellen Prozessen, ohne darauf einzugehen, dass diese Vorgänge von Mensch zu Mensch verschieden abgewandelt erscheinen. Sie spricht von Liebe, von Stimmungen usw. als Möglichkeiten der Seele, abgesehen von der Tatsache, dass es Menschen gibt, die der Liebe, der Heiterkeit usw. gar nicht fähig sind und die dadurch eine individuelle Eigenart, eine besondere Prägung ihrer seelischen Lebensart erhalten.

Sieht also die allgemein-psychologische Fragestellung ihren Gegenstand in der Allgemeinheit des Begriffes der menschlich-seelischen Wirklichkeit überhaupt, so stellt die charakterologische Betrachtung

[1] Der vorliegende Aufsatz ist entnommen aus dem Buch Ph. Lersch, *Der Aufbau des Charakters*, 1937.

das Seelenleben unter den Gesichtspunkt der complexen einzelmenschlichen Ausprägung. Ihr Grundbegriff ist derjenige des *Charakters*, der durch sie eine besondere Auslegung erhält; denn im deutschen Sprachgebrauch ist Charakter kein ausschliesslich seelenkundlicher Begriff.

Abgeleitet von dem griechischen Wort χαράσσειν = ritzen prägen, wird er in weitester Bedeutung dann gebraucht, wenn wir etwa von dem Charakter der italienischen Landschaft oder vom Charakter Bach'scher Musik sprechen. Wir meinen damit die gestalthafte Eigenart, durch die sich für uns die italienische Landschaft etwa von der deutschen, die Musik Bachs von derjenigen Wagners unterscheidet —eine Eigenart, die wir, bis zu einem gewissen Grade wenigstens, begrifflich zu erhellen und sprachlich zu formulieren vermögen. In dieser weitesten Bedeutung findet der Charakterbegriff Anwendung auf Erscheinungen überhaupt; man kann im Hinblick hierauf von seinem ästhetischen Gebrauch sprechen, wenn man Ästhetik nicht im Sinne der klassischen Kunsttheorie als Lehre vom Schönen versteht, sondern in dem erweiterten Sinn, in dem die Romantik das Charakteristische, die gestalthafte Eigenprägung einer Erscheinung als Grundbegriff in die Ästhetik eingeführt hat.

In wiederum anderer und zwar wesentlich engerer Bedeutung erscheint der Charakterbegriff in der oft gebrauchten Wendung, es sei einer ein "Mensch von Charakter." Charakter hat hier den Sinn eines sittlichen Wertes und wird *den* Menschen zugesprochen, die in ihrer Willenshaltung und in ihrer Gesinnung so durchorganisiert sind, dass sie zwei Grundeigenschaften aufweisen: volle Verantwortung für ihr Handeln und Folgerichtigkeit und damit Zuverlässigkeit ihrer Verhaltens. Die Treue zu sich selbst, die Festigkeit und Eindeutigkeit der Lebensführung sind die Grundmerkmale, die—als sittliche Werte gesehen—überall da gemeint sind, wo von "Charakterbildung" und von "Charaktererziehung" gesprochen wird.

Zwischen dem ästhetischen Charakterbegriff als dem weitesten und dem ethischen als dem engsten liegt der seelenkundliche insofern in der Mitte, als er sich seinem Umfang nach nicht—wie im ästhetischen Gebrauch—auf Erscheinungen überhaupt, sondern—wie im ethischen Gebrauch—nur auf die Erscheinung des Menschen als eines seelischen Wesens bezieht. Anderseits ist er weiter als der ethische Charakterbegriff; denn er betrachtet nicht wie dieser den Menschen unter dem sittlichen Ideal einer Forderung, wie er sein soll, sondern als das, was er ist. Menschen, die im ethischen Sinn charakterlos sind, haben

seelenkundlich gesprochen durchaus einen bestimmbaren Charakter. Denn im seelenkundlichen Gebrauch bezeichnet der Begriff des Charakters die individuelle Eigenart des Menschen, wie er sich, aufsteigend aus den unbewussten Tiefen lebendiger Natur zur Bewusstheit menschlicher Existenz, fühlend und handelnd, in Willensentscheidungen, Wertungen und Zielsetzungen, in Urteilen und geistigen Stellungnahmen mit der Welt auseinandersetzt und dadurch ein erfassbares Gepräge seines individuellen Daseins erhält.

II. Das Wesen des Charakters

Freilich ist diese Bestimmung des Charakters noch durchaus vorläufig. Es besteht nunmehr die Aufgabe, genauer anzugeben, was es heisst, das menschliche Seelenleben als Charakter betrachten.

(*1*) Der erste Begriff, der hier von grundlegender Bedeutung wird, ist derjenige der *Disposition*. Seine Klarstellung bedarf einer besonderen Überlegung.

Das seelische Leben, wie es uns in der Selbsterfahrung unmittelbar gegeben ist, hat seine Wirklichkeit immer als Prozess, als Geschehen, das sich von Augenblick zu Augenblick wandelt. Es gibt keinen im strengen Sinn beharrenden Zustand.

Dennoch bietet das seelische Leben eines Menschen, im Längsschnitt der Zeit betrachtet, keineswegs das Bild eines chaotischen Durcheinanders und Vielerleis, das sich jedem Versuch einer Ordnung,Gliederung und eines Überblicks widersetzt. Vielmehr beobachten wir im Wechsel der gegebenen seelischen Zustände und Vorgänge eines Menschen eine Wiederkehr des Ähnlichen, eine gewisse Konsequenz und individuelle Gesetzmässigkeit. Vergleicht man verschiedene Lebensphasen eines Menschen hinsichtlich seiner Lebensgrundstimmung, seiner Zielsetzungen und Strebungen, der Richtung und Formen seines Denkens, seiner Entscheidungen und praktischen Verhaltungsweisen, so findet man im Wandel des mannigfaltigen Geschehens eine gewisse Gleichförmigkeit in der Art, wie die Erfahrungen des Lebens in der individuellen Daseinsentfaltung seelisch verarbeitet werden. So ist es für gewisse Menschen eigentümlich, dass sie auf Widerstände, die ihnen begegnen, mit einer Intensivierung ihres Wollens antworten, während andere ausweichen und nach Umwegen suchen, wieder andere ihr Ziel überhaupt aufgeben und resignieren. Sofern solche Erlebnisweisen sich als relative Konstanten im Seelenleben des Einzelnen feststellen lassen, bezeichnen wir sie als seelische Dispositionen.

Wenn wir also in der Seelenkunde von Charakter sprechen, ge-

schieht dies auf dem Hintergrund einer Unterscheidung zwischen dem *aktuellen* Seelenleben, den je und je gegebenen seelischen Vorgängen und Zuständen, und den verhältnismässig gleichbleibenden, von Mensch zu Mensch verschiedenen *habituellen* Bereitschaften zu bestimmten Arten solcher Vorgängen und Zustände, also zu bestimmten Formen, sich seelisch zu verhalten, seelisch zu reagieren. Die Dispositionen werden gedacht als habituelle Eigenschaften der Erlebnisweise, die im Wandel des Geschehens wiederkehren und das seelische Verhalten im Hier und Jetzt bestimmen. Das Verhältnis, in dem hierbei die aktuellen seelischen Ereignisse zu den habituellen Bereitschaften, den Dispositionen stehen, wird am besten dadurch ausgedrückt, dass wir sagen: die Dispositionen *erweisen* sich im aktuellen seelischen Leben.

Die Dispositionen sind es, durch die das Seelenleben eines Menschen, im Längsschnitt der Zeit betrachtet, ein gewisses Gepräge erhält. Das Seelenleben als Charakter betrachten heisst also zunächst, es betrachten als Gepräge, das bestimmt wird durch das Vorhandensein von Erlebnisformen und Bereitschaften, die sich im Fluss der ständig wechselnden seelischen Vorgänge und Zustände als verhältnismässig konstant erweisen.

(2) Die Bestimmung des Charakters als das Insgesamt von seelischen Dispositionen (Eigenschaften) ist jedoch noch keineswegs zureichend. Denn genauer besehen ist das Zusammensein seelischer Dispositionen im Verband eines Charakters von besonderer Art. Da der Charakter eine lebendige Wirklichkeit ist, ist auch für ihn der *Integrationszusammenhang* d.h. die gegenseitige Abhängigkeit der unterscheidbaren Teile und die wechselseitige Durchdringung ihrer Funktionen eigentümlich. Die Dispositionen stehen als unterscheidbare Einzelzüge des Charakters zueinander in einem inneren Zusammenhang, sie durchdringen einander in ihrer Wirksamkeit. Alle aufweisbaren Einzelzüge sind durch die Geschlossenheit in der Einheit der Person voneinander abhängig, sie beeinflussen einander, färben aufeinander ab, kurz: sie stehen in einer eigenartigen Beziehung zueinander, sind also niemals voneinander isoliert; jeder Einzelzug hat seine Bedeutung für den anderen.

Aus der Tatsache des grundsätzlichen Integrationszusammenhanges der abhebbaren Einzelzüge des Charakters ergibt sich mit Notwendigkeit, dass gewisse Dispositionen (Charaktereigenschaften) zueinander im Verhältnis der inneren Verwandtschaft stehen bzw. einander ausschliessen. Stellt man die Frage, ob der ehrgeizige Streber mitleidsfähig sei, so wird sie aus einer inneren Logik heraus verneint werden müssen.

Es ist selbstverständlich, dass ehrgeiziges Strebertum und Mitleidsfähigkeit kaum auf einem und demselben Charakterstamm wachsen; oder, ins Positive gewendet: wie ehrgeiziges Strebertum und Mitleidsfähigkeit kaum in derselben Charaktereinheit vorzufinden sind, so sind anderseits Strebertum und Hartherzigkeit im Hinblick auf ihr Vorkommen in einem Charakter miteinander wesensverwandt oder *affin*. Umgekehrt sind Strebertum und Mitleidsfähigkeit einander charakterologisch wesensgegensätzlich oder *diffug* (diffugium = das Auseinanderfliehen). Auch Hass und phlegmatisches Temperament sind charakterologisch diffug, ebenso Gutmütigkeit und Verlogenheit, aber auch Gutmütigkeit und Neid. Den Rahmen möglicher Charaktereigenschaften, der durch das Vorhandensein einer bestimmten Eigenschaft auf Grund der charakterologischen Affinitäten vorgegeben ist, nennen wir das charakterologische *Umfeld* jener Eigenschaft. Die Absteckung des charakterologischen Unfeldes einer eindeutig ermittelten Eigenschaft ist für die praktische Charakterbestimmung ein wichtiges forschungstechnisches Prinzip.

(3) Die Eigenschaften (Dispositionen) stehen als abhebbare Einzelzüge in der Einheit eines Charakters nun nicht nur im Verhältnis der gegenseitigen Durchdringung und Wechselwirkung, sondern darüber hinaus in einem bestimmten Ordungsverhältnis. Es gibt ein ordnendes Prinzip, das den Einzelzügen ihren Stellungswert und Sinn in der Einheit eines bestimmten Charakters verleiht. Diese Anordnung der Einzelzüge, ihr Verhältnis der Über- und Unterordnung nennen wir die *Struktur* oder das Gefüge eines Charakters. Wenn sich feststellen lässt, dass ein Mensch ehrgeizig, humorlos, verdrossen, unfähig zur Hingabe, arbeitsam und willenszäh ist, so ist es einsichtig, dass diese Einzelzüge in Affinität zueinander stehen. Aber es genügt nicht, ihre innere Zusammengehörigkeit und Verwandtschaft zu sehen, sondern es ist in dem besonderen Fall, der hier ins Auge gefasst wird, darüber hinaus festzustellen, dass alle diese Einzelzüge in einer gefügehaften Anordnung stehen: der Ehrgeiz ist gleichsam das Grundmotiv des Charakters, der rote Faden, der durch alle seine unterscheidbaren Einzelzüge geht, er spielt die Rolle eines strukturierenden Prinzips, aus dem die übrigen Eigenschaften verständlich werden.

Durch die Struktur als das organisierende Prinzip, das die in innerer Verwandtschaft und Wechselwirkung stehenden Einzelzüge eines Charakters zueinander in das Verhältnis der Über- und Unterordnung bringt, erhält die Einheit des Charakters die besondere Art der individuellen *Ganzheit*. Das heisst aber nichts anderes, als dass die abheb-

baren Einzelzüge in ihrem Sinn und Wesen nur vom Ganzen her zu verstehen sind. Die Einzelzüge lassen sich immer nur abheben vom Ganzen, sie bleiben in ihrer Wirklichkeit aber immer eingebettet in den Zusammenhang des Ganzen. Wie das Ganze eines Baumes sich in Wurzeln, Stamm, Asten, Zweigen und Blättern ausgliedert und diese Teile gerade vom Ganzen her ihren Sinn bekommen, so stehen auch die unterscheidbaren Einzelzüge eines Charakters—die spezifischen Stimmungen und Gefühle, Affekte und Leidenschaften, Triebe und Strebungen, Gedanken und Vorstellungen, Entschlüsse und Handlungen—miteinander in einem sinnvollen Zusammenhang und können in ihrem Wesen niemals verstanden werden, wenn man sie loslöst vom gegliederten Ganzen, das wir den Charakter nennen.

Nun darf freilich mit dem Begriff der ganzheitlichen Struktur des Charakters nicht die Vorstellung einer notwendigen Harmonie, eines Zusammenklanges und einer Ausgewogenheit der einzelnen Züge verbunden werden. Vielmehr lässt das formale Prinzip der Ganzheit des Charakters durchaus die Tatsachen des Widerspruchs, der gegenseitigen Spannung und Auseinandersetzung zu. Ja, diese Tatsachen sind überhaupt nur möglich unter der Voraussetzung der ganzheitlichen Bezogenheit der Einzelzüge. "Denn es gäbe keinen internen Widerstreit der Tendenzen, wenn sie nicht Tendenzen eines und desselben Interessensubjektes, eines und desselben Individuums, wenn sie nicht momentane oder dauernde "Züge" eines einheitlichen Charakters wären. Gerade ihre gegenseitige Spannung ist Ausdruck der Einheit in der Mannigfaltigkeit; ohne sie könnten Tendenzen zwar verschieden sein, aber sie könnten nicht einen internen "Widerspruch" (Widerstreit) bedeuten."[2]

Aus der Tatsache des Strukturzusammenhanges ergibt sich für die praktisch empirische Bestimmung eines Charakters eine wichtige Aufgabe, die wir die strukturelle Aufhellung nennen. Sie zielt darauf ab, die ermittelten Eigenschaften eines Charakters in die Beziehung der Über- und Unterordnung zu bringen, ihren Stellungswert im Charakterganzen zu bestimmen. Wenn wir zwei Menschen vorfinden, die mit grosser Intelligenz ausgestattet, also in gleichem Maasse befähigt sind, kritisch und analytisch scharf zu denken und Beziehungen zu erfassen, so hat die strukturelle Aufhellung festzustellen, welche Wertigkeit der intellektuellen Ausstattung im Ganzen der beiden Charaktere zukommt. Hier wird sich zeigen, dass der eine etwa

[2] Paul Häberlin, *Der Charakter*, 1925, 45.

Intelligenz um ihrer selbst willen gebraucht; es befriedigt ihn, sich die Welt mit Hilfe seines Verstandes klarzulegen, der Sinn seines Lebens erfüllt sich in der Gewinnung rein theoretischer Einsichten. Für andere Menschen dagegen hat ihre gute Intelligenz eine ganz andere strukturelle Wertigkeit. Sie sind von starkem Geltungsstreben oder Machthunger vorwärtsgetrieben und eben diesem Streben erweist sich die Intelligenz als ein zweckmässiges und erfolgreiches Instrument. Während im ersten Falle die Intelligenz gleichsam sich selbst genügt und damit eine zentrale Rolle im Ganzen des Charakters spielt, erscheint im zweiten Falle die Intelligenz an einer ganz anderen Stelle in der Persönlichkeit, in einer wesentlich anderen strukturellen Wertigkeit. Grundsätzlich ist also zu sagen, dass hinter derselben Charaktereigenschaft bei verschiedenen Menschen völlig verschiedene Strukturzusammenhänge stehen, dass sie einen unterschiedlichen strukturellen Sinn haben können.

Zusammenfassend sollen noch einmal der Begriff des Charakters, wie er in der Seelenkunde zu verstehen ist, bestimmt und die wichtigsten Wesensmerkmale, die ihm zukommen, in das richtige Verhältnis gebracht werden. Unter Charakter verstehen wir das ganzheitliche Gefüge von Erlebnisformen (Dispositionen), die als relativ beharrende Eigenschaften der Wirklichkeit eines Menschen als eines seelischen Wesens ein begrifflich bestimmbares Gepräge verleihen. Dabei denken wir die Dispositonen (Eigenschaften) als Merkmale der individuellen Eigenart des seelischen Lebens, die sich in der Mannigfaltigkeit der zeitlich sich entfaltenden und immer wechselnden einmaligen seelischen Vorgänge und Zustände als relative Konstanten erweisen. Wir gewinnen ihre Erkenntnis immer nur im Rückschluss vom aktuellen Seelenleben und seinen Äusserungen. Die Eigenschaften des Charakters stehen—ebenso wie auch die aktuellen Einzelvorgänge und Zustände des Seelenlebens—zueinander im Verhältnis der gegenseitigen Durchdringung (Integration) d.h. sie hängen in ihrer Wirksamkeit innerlich zusammen bezw. voneinander ab. In diesem Integrationszusammenhang lässt sich nun ein Verhältnis der Über- und Unterordnung der abhebbaren Einzelzüge feststellen; dieses Ordnungsverhältnis nennen wir die Struktur des Charakters. Dank der Struktur ist die Einheit des Charakters nicht die einer *Summe*, die sich aus Einzelteilen *zusammensetzt*, sondern diejenige der *Ganzheit*, die sich in den unterscheidbaren Einzelzügen *ausgliedert*. Die Einzelzüge erhalten ihre Wertigkeit aus ihrem Eingebettetsein in das strukturierte Ganze; sie sind von ihm aus zu verstehen.

III. Die Problemkreise der Charakterkunde

Der Charakter im seelenkundlichen Sinn ist Gegenstand der Wissenschaft, die sich unter dem Namen der Charakterkunde (Charakterologie) in den letzten Jahrzehnten immer mehr erweitert und methodisch gefestigt hat. Charakterkunde ist der Inbegriff allgemeiner Sätze, die sich—ausgehend von der Erfahrung—über die Erscheinungsformen und die Gesetzmässigkeiten des Charakters aussagen lassen.

(*1*) Ein erster Problemkreis ist gegeben durch die Frage nach den wesentlichen Bestimmungsmerkmalen, aus deren ganzheitlichem Gefüge sich das Gepräge eines Charakters ergibt. Man sucht nach einer eindeutigen Ordnung von Gesichtspunkten, unter denen wir den Einzelmenschen befragen müssen, um die wesentlichen Merkmale seines Charakters in den Griff zu bekommen und seine Eigenart der Aufhellung und Bestimmung zugänglich zu machen. Wir nennen diesen Problemkreis den des *Charakteraufbaus*.[3]

(*2*) Ein zweiter Problemkreis beschäftigt sich mit der Frage nach *Typen* des Charakters, d.h. nach Grundformen seelischer Eigenart, die verschiedenen Menschen in vergleichbarer Weise zukommen. Diese Fragestellung ergibt sich aus der Eigenart der Charakterkunde als einer Wissenschaft mit innerer Folgerichtigkeit. Wissenschaft hat ja immer die Aufgabe, die Mannigfaltigkeit der empirisch gegebenen Einzelerscheinungen überschaubar zu machen, sie versucht von der individuellen Einmaligkeit eines Falles abzusehen und die Bestimmungsmerkmale zu finden, die die einzelnen Fälle miteinander vergleichbar machen. Der Begriff des Typus in der Charakterkunde bedeutet nichts anderes als die Verallgemeinerung von Strukturen, also von gegliederten Ganzheiten unterscheidbarer Persönlichkeitsmerkmale.[4]

(*3*) Ein dritter Problemkreis der Charakterkunde ergibt sich aus der Frage nach den *Entstehungsbedingungen* (Genese) des Charakters. Es handelt sich hier darum, zu entscheiden, welche Eigenschaften des Charakters bedingt sind durch die naturgegebenen, ererbten Anlagen (endogene Faktoren) und wie weit anderseits das Gepräge der Persönlichkeit in seinen Einzelmerkmalen bestimmt ist durch Bedingungen, die von aussen an den Menschen herantreten (exogene Faktoren). Das Insgesamt der Bedingungen ersterer Art nennen wir die

[3] Ein solches System des Charakteraufbaus entwickelt Ludwig Klages, *Die Grundlagen der Charakterkunde* (6. Aufl.), 1928, 1-233; ferner Philipp Lersch, *Der Aufbau des Charakters*, 1937.

[4] Vgl. Philipp Lersch, Probleme und Ergebnisse der charakterologischen Typologie, *Ber. ü. d. 13. Kongr. d. dtsch. Ges. f. Psychol.*, in Leipzig, 1933, 76-97.

seelische *Konstitution*; sie stellt gleichsam das Apriori der Lebensentfaltung und Lebensgestaltung dar, mit der der Charakter eines Menschen zur Erscheinung kommt. Die Bedingungen der zweiten Art hat man zusammengefasst unter dem Begriff der *Umweltseinflüsse*; besser wäre vielleicht der Begriff der *Begegnungen*. Es fallen darunter alle Einflüsse der individuellen durch das äussere Schicksal bedingten Lebenslagen, der Erziehung, des gesellschaftlichen Milieus, des Elternhauses, der Landschaft, usw., durch die der Charakter eines Menschen seine Prägung erhält.

Dass die Frage nach den Entstehungsbedingungen eines Charakters nicht nur theoretisches Interesse besitzt, sondern praktisch einerseits für die Probleme der *Erziehung* und anderseits für die der erbbiologischen *Züchtung* weittragende Bedeutung hat, ist klar. Alle Erziehung hat ihren Spielraum nur innerhalb der anlagemässigen Bedingungen eines Charakters. Sie wird deshalb, soweit die Ergebnisse der Konstitutionsforschung es nicht verbieten, die anlagemässigen Bedingungen eines Charakters auf ein hypothetisches Minimum beschränken. Die Erbcharakterologie ihrerseits wird die natürlichen Grenzen der Erziehung immer weiter festlegen und zwar in dem Maasse, in dem sie sich ihrem Ziel nähert, diejenigen Eigenschaften zu ermitteln, die anlagemässig bedingt und einer Änderung durch Beeinflussung von aussen nicht fähig sind. Ein wertvolles Hilfsmittel auf diesem Wege hat sie u.a. im Material der eineiigen Zwillinge, also Menschen gleicher Erbanlage, sofern ihr Leben sich in ganz verschieden gearteten äusserlichen Verhältnissen abspielt.[5]

Freilich wird die Erbcharakterologie billigerweise sich nie erhoffen dürfen, den Menschen in der Allseitigkeit seiner Prägung als Charakter aus Bedingungen der Anlage erklären zu können. Denn wenn auch die Forschungen noch durchaus im Flusse sind, soviel erscheint schon heute unbezweifelbar, dass an dem Gepräge einer Persönlichkeit sowohl Anlage als auch Begegnungen, Umweltseinflüsse beteiligt sind. Aufgabe der Erbcharakterologie kann es immer nur sein, zu entscheiden, welche Grundeigenschaften eines Charakters anlagebedingt sind.[6]

(*4*) Von der Frage nach der Entstehung (Genese) des Charakters ist diejenige nach seiner *Entwicklung* zu unterscheiden. Es handelt sich hier um die charakterologisch ganzheitliche Bestimmung der einzelnen

[5] Vgl. hierzu F. Lange, *Verbrechen als Schicksal*, 1929.

[6] Vgl. Gerhard Pfahler, *Vererbung als Schicksal: Eine Charakterkunde*, 1932, 1-234; H. Hoffman, *Das Problem des Charakteraufbaus: Seine Gestaltung durch die erbbiologische Persönlichkeitsanalyse*, 1926, 1-193; Bruno Petermann, *Das Problem der Rassenseele*, 1935, 1-230.

Entwicklungsabschnitte des menschlichen Lebens, wie wir sie als Phasen der Kindheit, des Jugendalters, der abgeschlossenen Reife und des Greisenalters gegeneinander abgrenzen. Sie alle haben ein charakterologisch eigentümliches Gepräge, das die individuelle Prägung des Einzelmenschen überschichtet. Praktisch legt diese Tatsache die Verpflichtung auf, sich zu fragen, wie weit gewisse Züge der Persönlichkeit nicht eigentlich als individuelle Eigenart, sondern lediglich als typische Merkmale des Lebensalters, der augenblicklichen seelischen Entwicklungsphase zu verstehen sind, sodass mit ihrer Änderung bei weiterschreitender Entwicklung zu rechnen ist. Dies muss vor allem bei der Beurteilung jugendlicher Charaktere berücksichtigt werden.

(ε) Ein fünfter Aufgaben- und Forschungskreis der Charakterkunde ist derjenige der *Diagnostik*. Hier wird gefragt nach den Wegen, die es ermöglichen, Kenntnis von einem in der Erfahrung begegnenden Charakter zu erlangen. Denn der Charakter eines Menschen ist unserer Erkenntnis nicht unmittelbar erschlossen, sondern es bedarf immer einer gewissen Schulung und Erfahrung, um ihn aus dem, was uns am anderen Menschen zugänglich ist, also aus seiner Erscheinung, seinen Äusserungen und Verhaltungsweisen richtig zu ermitteln.

Die Formen, in denen seelische Eigenschaften anderer Menschen unserer Erkenntnis zugänglich werden, sind mannigfaltig. Es ist methodisch wichtig, zunächst einmal Mitteilung und Ausdruck als Äusserungsformen seelischen Lebens zu unterscheiden.

(*a*) Die *Mitteilung* vollzieht sich im Mittel der Sprache—im weitesten Sinne der Zeichensprache verstanden—und ist abhängig von einer inneren Bereitschaft, sich zu äussern und der besonderen Fähigkeit dazu, die sich erst auf einer gewissen Entwicklungsstufe zeigt. Verhältnismässig spät kann der Mensch sich mitteilen, vermag er zu sagen, was er denkt, was er will, was er fühlt oder von welchen Ideen er geleitet wird. In der seelenkundlichen Praxis bedient sich vor allem der Arzt des Mittels der Mitteilung und zwar in der Form der Aussprache des Patienten (Exploration).

(*b*) Eine grundsätzlich andere Art von Brücke, auf der die seelische Wirklichkeit aus ihrer Innenweltlichkeit übertritt in die Erscheinungssphäre der Aussenwelt, stellt die Äusserungsform dar, die wir *Ausdruck* nennen. Was für die Mitteilung wesentliches Merkmal ist, dass sie nämlich bewusst und willkürlich geschieht, unter der Leitung einer Absicht, sich dem anderen zu erschliessen, ist für das Wesen des Ausdrucks nebensächlich. Im Schrei des Entsetzens, in der Pulsbeschleunigung der Angst, im Erröten der Scham, im Weinen der Trauer und im Lachen der Fröhlichkeit fehlt jene in der Mitteilung gelegene Absicht, auf das seelische Erlebnis hinzuweisen und es der Kenntnis der Mitwelt zu vermitteln. Diese Absicht der Äusserung tritt in der Mitteilung zu dem seelischen Sachverhalt, der mitgeteilt werden soll, als etwas Selbständiges und Neues hinzu. Im Ausdruck dagegen stellt das Erlebnis mit dem sinnlich vernehmbaren Zeichen, in dem es sich der Kenntnis der Mitwelt erschliesst, eine Seinseinheit dar, deren Innen und Aussenseite koexistential miteinander verbunden sind.

Da die bewusste Mitteilung der eigenen Innenwelt völlig von der Bereitschaft des Einzelnen abhängt und ausserdem—auch wenn diese Bereitschaft besteht—noch der Gefahr der Selbsttäuschung und des Sich-Selbst-Missverstehens ausgesetzt ist, wird die richtige Deutung der Ausdruckserscheinungen und ihre systematische Erforschung zu einem zentralen mannigfaltig gegliederten Aufgabengebiet der charakterkundlichen Diagnostik.

In seinen Gegenstandsbereich gehören die Erscheinungen des bewegten Mienenspiels, der *Mimik* und die Prägungen die sich durch den wiederholten Ablauf mimischen Geschehens als bleibende Spuren im Gesicht verfestigt haben.[7] Auch die Gebärden, Gesten, Körperhaltungen—zusammengefasst unter dem Begriff der *Pantomimik*—werden zur Visitenkarte der seelischen Wesensart eines Menschen.[8] Als Ausdruck vermögen wir ferner zu verstehen die Mannigfaltigkeiten der Art, wie ein Mensch spricht—im Unterschied zu dem, was er sagt, also den Inhalten seiner Mitteilungen.[9] Hier erkennen wir Unterschiede des Tempos (langsam, rasch), des Verlaufs (gleichmässig fliessend—abgehackt stossend, gemessen—überstürzt), Verschiedenheiten der Lautstärke (matt—kräftig) u.a. mehr, die alle etwas aussagen über den Menschen, der spricht. Wir erkennen den Traurigen an der Mattigkeit seiner Stimme, den Schüchternen an der Zaghaftigkeit seines Sprechens, den Erregbaren an gewissen Schwingungen und unvermittelten Lautstössen, den seelisch Gehemmten an Stockungen und Verkrampfungen. Schliesslich zeigt sich die seelische Eigenart eines Menschen auch am lebendigen Gezüge seines *Verhaltens*, seines Benehmens und Umgangs, seiner Handlungen und seiner Lebensführung.

(ε) Die bis jetzt genannten Ausdrucksvorgänge lassen sich unter dem Begriff der unmittelbaren Ausdruckserscheinungen zusammenfassen, sofern sie uns unmittelbar in ihrem lebendigen Vollzug am Menschen selbst begegnen. Aber nicht allein *sie* tragen ein charakterologisch deutbares Antlitz; charakterologisch verstehbar sind auch gewisse gegenständlich gewordene Äusserungen, die sich vom persönlichen Träger des seelischen Lebens losgelöst haben und am besten als *Werkgestaltungen* bezeichnet werden. Hierher gehört das stehende Schriftbild, an dem die lebendigen Bewegungen der Hand im Akt des Schreibens ablesbar sind und um dessen Deutung sich die Graphologie bemüht,[10] ferner jede Art geistiger Gestaltung, gleichviel ob es sich handelt um einen Aufsatz oder eine Zeichnung des Schülers, oder um die in der historischen Überlieferung erhaltenen und sich erhaltenden Kulturgüter der Malerei, Plastik und Architektur, der Dichtkunst und der Musik, der Weltanschauungen, Religionen, Staats- und Gemeinschaftsformen u. a. Man könnte diese Werkgestaltungen als *mittelbaren* Ausdruck bezeichnen; von den unmittelbaren Ausdruckserscheinungen unterscheiden sie sich dadurch, dass sie nicht nur zurückverweisen auf den Träger seelischen Lebens, sondern einen objektiven überindividuellen Sinn beanspruchen. Sie wollen verstanden werden als selbständige, autochthone Wirklichkeiten überpersönlicher Art. Ihre charakterologische Deutbarkeit ist gleichsam ihr zweites Gesicht, das hinter dem objektiv selbständigen Sinngehalt durchscheint. So erzählt Ludwig Richter in seinen Lebenserinnerungen, wie er in Tivoli einmal als junger Mensch, zusammen mit drei Kameraden einen Ausschnitt der Landschaft zu malen unternahm, er und die anderen fest entschlossen, von der Natur dabei nicht um Haaresbreite abzuweichen.

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[7] Vgl. Philipp Lersch, *Gesicht und Seele: Grundlinien einer mimischen Diagnostik*, 1932, 1-167.

[8] Vgl. H. Strehle, *Analyse des Gebarens: Erforschung des Ausdrucks der Körperbewegung*, 1935, 1-197.

[9] Vgl. I. B. Rieffert, Sprechtypen, *Ber. ü. d. 12. Kongr. d. dtsch. Ges. f. Psychol.*, 1932, 409-413.

[10] Vgl. Ludwig Klages, *Handschrift und Charakter* (11 Aufl.), 1928, 1-258.

Und obwohl nun das Vorbild das gleiche war und jeder mit gutem Talent an das sich gehalten hatte, was seine Augen sahen, kamen doch vier ganz verschiedene Bilder heraus, so verschieden unter sich, wie eben die Persönlichkeit der vier Maler. Woraus dann der Berichterstatter den Schluss zog, dass es ein objektives Sehen nicht gäbe, dass Form und Farbe je nach dem Temperament immer verschieden aufgefasst werden würden.[11]

(*d*) Von Mitteilung, unmittelbarer Ausdruckserscheinung und dem mittelbaren Ausdruck der Werkgestaltungen ist schliesslich eine vierte Form zu unterscheiden, in der uns die seelische Eigenart eines Menschen erkennbar wird: das *Symptom*. Das Eigentümliche des Symptoms liegt darin, dass wir von ihm auf seelische Sachverhalte *schliessen*. Der Zusammenhang, der zwischen dem Symptom als äusserem Zeichen und dem darin angezeigten seelischen Sachverhalt besteht, ist nicht ohne weiteres selbstverständlich und einsichtig wie etwa in der Ausdruckserscheinung des Erschreckens das sich ausdrückende seelische Zumutesein, sondern er wird lediglich auf Grund früherer Erfahrungen als bestehend angenommen. Das Symptom hat kein Gesicht, wie die unmittelbaren Ausdruckserscheinungen und die Werkgestaltungen, bei denen in dem aussenweltlich gegebenen Erscheinungsbild die seelische Innenwelt transparent und ablesbar wird. Ein Symptom deuten wir, wenn wir etwa in einer entlegenen, menschenleeren Gegend eine ausgebrannte Herdstelle finden und den Schluss ziehen, dass hier Menschen gewesen sein müssen, in denen Absichten, Überlegungen und Willensakte lebendig waren. Die Lehre vom Analogieschluss, mit der man das Verstehen von Ausdruckserscheinungen zu erklären suchte, hat gerade *nicht* im Bereich des (unmittelbaren und mittelbaren) Ausdrucks Geltung, sondern lediglich im Bereich der Symptome. Symptome werden gedeutet in *Analogie* zu den bisherigen Erfahrungen. In diesem Sinne sind etwa die plumpen, verkniffenen Züge des Mixödems Symptome geistig-intellektueller und seelischer Stumpfheit. Der Verzweiflungsschrei höchster Lebensgefahr als Ausdruck wird dagegen unmittelbar d.h. ohne Rückbeziehung auf Erfahrungen verstanden, sobald wir ihn zum ersten Mal vernehmen. Im Symptom drückt sich ein seelischer Sachverhalt nicht aus, sondern er *zeigt* sich *an*. Das Symptom liegt ausserhalb des Ausdruckszusammenhänge.

Die Unterscheidung zwischen Symptom und Ausdruck wird vor allem wichtig auf dem Gebiet der *Physiognomik*, d.i. der Lehre von der Erkennbarkeit seelischer Eigenart aus *architektonischen* Merkmalen der Leiberscheinung, die—verglichen mit den Bewegungsvorgängen der Mimik und Pantomimik—das Kennzeichen der Festigkeit und Beharrung tragen und bestimmt sind durch den Knochenbau, das Gewebe und die Fettschicht. Was in den Versuchen zur Physiognomik—angefangen bei der dem Aristoteles zugeschriebenen und von dem Italiener J. B. Porta wieder aufgegriffenen Tierphysiognomik bis zu den Arbeiten von Lavater, Gall und Lombroso—über den Zeichenwert der verschiedenen Formen des Kinns, der Stirne, der Nase, der Hand usw. ausgesagt ist, kann diagnostisch lediglich den Anspruch einer Symptomatologie erheben, die nur durch eine sorgfältige Erfahrungsstatistik zu begründen ist. Tatsächlich aber ist in den genannten Versuchen zur Physiognomik erprobte Erfahrung mit willkürlicher Phantasterei und voreiliger Analogiebildung kritiklos gemischt. Erst in der Gegenwart hat die Physiognomik in der auf den Tatsachen der Blutdrüsenforschung aufbauenden *konstitutions*typologischen Untersuchungen Kretschmers durch eine ausgedehnte Korrelationsstatistik eine solidere Grundlage gewonnen.[12] Aber auch bei diesen Forschern erscheint das architektonische Gepräge des Leibes lediglich als Symptom, d.h.d ie Zusammengehörigkeit von leiblichen und seelischen Merk-

[11] E. Wölfflin, *Kunstgeschichtliche Grundbegriffe*, 1918, 1 ff.
[12] Ernst Kretschmer, *Körperbau und Charakter* (7 Aufl.), 1929, 1-233.

malen wird rein tatsächlich aus der Erfahrung festgestellt, ohne dass ein innerer verstehbarer Zusammenhang zwischen leiblicher und seelischer Eigenart aufgewiesen würde. Es handelt sich also um eine Zuordnung ganz anderer Art, als sie im Verstehen von Ausdrucksvorgängen und Ausdruckserscheinungen vorliegt. Das Symptom steht, wie schon betont, ausserhalb der Ausdruckszusammenhänge.

Damit soll nun keineswegs gesagt werden, dass die Architektonik der Leiberscheinung immer nur den Deutungswert eines Symptoms haben kann, sondern lediglich dies, dass sie bei den genannten Forschern als Symptom aufgewiesen wird. Es ist jedoch durchaus möglich, gewisse Formen der architektonischen Leiberscheinung in einen Zusammenhang mit Ausdrucksvorgängen zu bringen und zwar insofern, als die Eigenart des Körperbaus als die vorgegebene Bahn verstanden werden kann, in der sich nur ganz bestimmte Ausdrucksvorgänge zu vollziehen vermögen. So lässt sich der Körperbau des Pyknikers zweifellos betrachten als das vorgebene Geleise, in dem sich nur Ausdrucksbewegungen vollziehen können, für die die Merkmale des Flüssig-Runden eigentümlich sind, Bewegungen also, denen jeder Zug des Stossartigen, Eckigen und Ruckhaften fehlt. In dieser Deutung der morphologischen Eigenart der Leib-Erscheinung, die weitgehend in der Rassenseelenkunde von L. F. Clauss[13] ihre Anwendung findet, wird das Architektonische nicht als Symptom verstanden, sondern, wie wir sagen wollen, als *virtueller Ausdruck*.

(*c*) Mit dem Begriff des virtuellen Ausdrucks, mit dem wir eine fünfte Art von deutbaren äusseren Zeichen seelischer Eigenart herausstellen, ist gemeint der diagnostische Wert einer gestalthaften Leiberscheinung, sofern sie die zureichende Bedingung für den Vollzug bestimmter Ausdrucksbewegungen, also gleichsam ihr vorgegebenes Geleise darstellt.

(*6*) Als letzter Aufgabenkreis der Charakterkunde ist die *Kasuistik* zu nennen, d.i. die Sammlung konkreter Einzelfälle und ihre wissenschaftlich systematische Verarbeitung. Denn als Erfahrungswissenschaft kann sich die Charakterkunde nur dann fruchtbar entwickeln, wenn sie sich hütet, voreilig zu verallgemeinern und zu systematisieren, und sich bereit hält, an neuen Erfahrungen ihre schon gewonnenen Erkenntnise zu überprüfen bzw. zu ergänzen. Kasuistische Sammlung und theoretische Verarbeitung müssen einander in die Hand arbeiten: der Charakterforscher muss—in Ehrfurcht vor der Erfahrung—immer bereit sein, am Einzelfall das System zu korrigieren. Anderseits darf er sich nicht damit begnügen, den Einzelfall lediglich zu beschreiben, ohne ihn auf allgemeine Gesichtspunkte über den Aufbau des Charakters und seine Grundformen zu beziehen.

Quellen der Kasuistik sind die Erfahrungen des täglichen Lebens in Familie, Beruf, Gemeinschaft und Gesellschaft, die Darstellungen der Biographien und Selbstbiographien, die Fälle der Psychopathologie und Kriminalistik und nicht zuletzt die Werke der National- und Weltliteratur. Die Menschendarstellungen der genialen Dramatiker und Epiker sind es vor allem, in denen die Charakterkunde ihre "exemplarischen Fälle" findet.

[13] L. F. Clauss, *Rasse und Seele: Eine Einführung in den Sinn der leiblichen Gestalt* (3 Aufl.), 1933.

IV. Die Bedeutung der Charakterkunde

Schliesslich noch ein Wort über Bedeutung und Sinn der Charakterkunde. Sie—wie überhaupt die Seelenkunde—entbehrte ihrer Berechtigung, entspränge sie lediglich einer intellektuellen Neugier oder dem abwegigen Genuss, im eigenen und fremden Seelenleben analysierend zu wühlen. Ihre Rechtferigung erhält sie nur daraus, dass sie dem Leben, seiner Gestaltung und Sinnerfüllung dient. Wird sie von dieser Gesinnung des Ernstes und der Verantwortung getragen, dann erhält sie unmittelbare praktische Bedeutung. So mannigfaltig die Erscheinungen des gesellschaftlichen, beruflichen, staatlichen und privaten Lebens sind, so mannigfaltig sind die Formen, in denen die Charakterkunde wichtig wird. Charakterologische Tatsachen und Zusammenhänge durchziehen den Erfahrungskreis des Alltags, das menschliche Zusammenleben in den Organisationen der Gesellschaft und den Formen der Gemeinschaft von Familie, Volk und Staat. Sie begegnen dem Arzt, dem Seelsorger, dem Erzieher, dem Politiker und dem Richter in den besonderen Erfahrungskreisen ihrer Berufe. Vor allem auch auf dem Gebiet der Berufsberatung und der Berufsauslese kommt heute der Charakterkunde eine wichtige Rolle zu, nachdem man eingesehen hat, dass die Bewährung eines Menschen im Beruf nicht nur bestimmt wird durch blosse Fertigkeiten und Tauglichkeiten, sondern dass der ganze Mensch mit in die Berufsleistung eingeht.

Nun ist zweifellos zuzugeben, dass in den genannten Lebenskreisen sich erfahrungsgemäss schon eine Form der Menschenkenntnis praktisch bewährt, die durchaus vorwissenschaftlicher Art ist. Man denke an jene Kunst der Menschenbehandlung, über die gute Politiker, Diplomaten und Geschäftsleute verfügen, an die Hellsichtigkeit der Einfühlung, die zuweilen beim Seelsorger, Arzt und Erzieher zu finden ist. Wenn man es in diesen Fällen oft mit sehr bedeutsamen Einsichten in allgemein psychologische und charakterologische Zusammenhänge zu tun hat, so unterscheidet sich jene praktische "Kennerschaft" von der wissenschaftlichen Charakterkunde dennoch in zweifacher Hinsicht. Die praktische, in der Menschenbehandlung sich bewährende Seelenkenntnis muss als solche keineswegs, wie es die Wissenschaft für sich fordert, in sprachlicher Formulierung und begrifflicher Klarheit demjenigen zu Bewusstsein kommen, der sie besitzt. Sie ist vielmehr meistens wirksam nur als unbewusste, "instinktive" Einsicht. Und wenn dann einmal solche Einsichten bewusst und ausgesprochen werden, dann geschieht es meist in der Form gelegentlicher Aphorismen, ohne den Anspruch ein überschaubares und zusammenhängendes Ganzes

von Erkenntnissen zu geben, also ohne die Absicht, die die Charakterkunde als Wissenschaft verfolgt. *Ihr* kommt es darauf an, in systematischer Ordnung Grundsätzliches über das Wesen, den Aufbau, die inneren Zusammenhänge und die möglichen Formen des Charakters auszusagen. Dabei geht sie mit überlegter Methode an die Erfahrung heran, unterscheidet sich also auch hierin von der vorwissenschaftlichen Einstellung praktischer Kennerschaft, die ihre Einsichten gewinnt, wie es die Gelegenheiten des Lebens gerade mit sich bringen.

Nun ist es keineswegs so, dass die Wissenschaft der Charakterkunde jene praktische Menschenkenntnis überflüssig macht. Im Gegenteil: die Erfahrung zeigt, dass eine Anwendung der Charakterkunde auf das Leben auch bei umfassender Kenntnis der wissenschaftlichen Methoden und Forschungsergebnisse zum Misserfolg führt, wenn sie nicht getragen wird vom Mutterboden jener intuitiven Begabung, die sich auch schon in den vorwissenschaftlichen Formen praktischer Menschenkenntnis und Menschenbehandlung bewährt und die in der Charakterkunde ihre methodische und systematische Ergänzung und Erhellung findet.

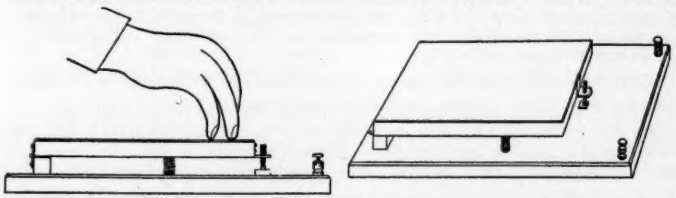
REACTION-TIME AND MOTOR DEVELOPMENT

By Harold E. Jones, University of California
Editor, *The Journal of Educational Psychology*
(Founded in 1910)

As a part of a study of motor development,[1] tests of manual reaction-time to a sound stimulus have been administered seriatim to a normal sample of pupils in grades 6 to 10. The test group (consisting originally of 100 boys and 100 girls) was chosen in 1932 from the fifth grades of six elementary schools; availability of the subjects for cumulative testing was the chief criterion for selection. The sample comprises a representatively wide range of intelligence and social status, with a mean IQ slightly above that of the city average.

Procedure

A Cenco synchronous motor chronoscope, recording in units of 1/120 of a second, was used for this series of tests. Although not adequate for some types of precise measurement, it is well adapted to the study of individual differences in simple reaction-time.[2] With the



Side View (*E*) Top View

Fig. 1. Reaction-Key

[1] Acknowledgments are due to WPA project No. 4428 for clerical and statistical assistance, to the Oakland Public Schools, and particularly to Mr. J. J. Rulon for assistance in various stages of the investigation. The present report deals with one phase of a longitudinal study of adolescents, undertaken by the Institute of Child Welfare of the University of California with the aid of a subvention from the Laura Spelman Memorial Fund. The aim of the principal project, which has utilized the coöperating services of psychologists, physiologists and physicians, has been to describe and analyze certain of the characteristic features of development during adolescence. Cumulative measures have been obtained of numerous specific aspects of gross morphology, physiological function, simple and complex motor reaction, physical ability, intellectual functions, and social and emotional characteristics. Attention has been directed primarily to the latter two categories, and to the preparation of both qualitative and quantitative records which can be used in the study of interrelationships.

[2] W. R. Miles, The correlation of reaction and coördination speed with age in adults, this Journal, 43, 1931, 377–391.

aid of a supplementary re-set device (supplied by R. H. Seashore) readings can be taken directly from the re-set dial, which rotates outside the chronoscope and is graduated in the same units.

The chronoscope is operated on 60-cycle alternating current, through a step-down transformer adjustable within a range of from 5.5 to 16.5 volts. The chronoscope is connected in series with *E*'s switch and with *S*'s key, which consists of a hinged spring-board 4 in. wide and 6 in. long (see Fig. 1). At the beginning of each test, *S* is instructed to rest the finger tips of the right (or preferred) hand upon the upper end of the spring-board, the four fingers being slightly flexed and the hand clear of the board. The position of the hand is illustrated by *E*. By light pressure, equivalent to a weight of 12 oz., a contact beneath the board is closed. In work with children, a manual switch of this type, operated by a total hand reaction, involves less tension during the preparation period and is easier to administer than a telegraph key actuated by one finger. The effect of the type of motor reaction is reported in a later section.

E illustrates the fact that after the reaction-board has been depressed, closure of a concealed switch at *E*'s end of the table will start the chronoscope. The sound of the synchronous motor in the chronoscope is the signal to a withdrawal reaction (lifting fingers), which breaks the circuit and stops the chronoscope. A warning signal is provided by a red light which is illuminated at a variable short interval (1–3 sec.) prior to the starting of the clock. Fifteen records are taken with the right hand, followed by fifteen records with the left.

The following verbal instructions were given the *S*'s:

This is a test to see how fast you can move your hand. You place your hand on this board and hold it down: in a moment a light will appear in the red bulb. This is a signal that in 1, 2, or 3 sec. a buzzer in the clock will sound. When you hear that buzzer, you must lift your fingers off just as fast as you can (demonstrating). At the sound of the buzzer, a little electric clock starts; the clock stops when you lift your fingers, and by reading the dial we can tell how quick you are.

Beyond these instructions, no attempt is made to control the direction of *S*'s attention (*i.e.* upon the auditory stimulus, or upon the motor reaction).

Two fore-exercise trials are given. From this point on, the *S*'s are informed of their scores and attention is called to improvement or to lack of improvement. Preceding trials 5, 10, and 15, an attempt is made to provide increased motivation by urging *S* to break his record. The form in which this encouragement is given depends somewhat upon *E*'s judgment as to the particular procedure which will be most effective with the individual child. Substitute trials are given in the occasional instance when an obviously premature reaction occurs. In the computation of scores, medians are used throughout in order to reduce the influence of extremely long measures (which may represent distraction) or of extremely short measures (sometimes produced by a premature response synchronizing closely with the sound stimulus).

Results

Every *S* in the study received two preliminary tests, of from 20 to 30 trials each, designed to acquaint him with the apparatus and to standardize procedure and motivation techniques. Preliminary results indicated the importance of maintaining incentive by reporting the record for each trial and encouraging *S* to compete with his own

previous performance. In spite of the general interest in the test, average latent periods were shown to increase whenever a slackening occurred in the use of motivating devices. Although this is designated a test of "simple reaction-time," the functions involved are far from simple. Individual differences in performance are unquestionably influenced not merely by basic factors in the latent time of neuro-muscular mechanisms, but also by emotional factors, by competitive attitudes, and by many specific aspects of the subject-experimenter relationship.

TABLE I

MEAN REACTION-TIME (IN MS.) BY TRIALS (n = 163)

Trial	Mean of right hand		Mean of left hand	
	Obtained	Smoothed†	Obtained	Smoothed†
1	160.3	160.1	157.3	156.1
2	159.8	158.4	153.6	155.6
3	155.2	155.5	156.0	155.4
4	151.5	151.9	156.6	155.3
5*	149.0	150.5	153.2	153.3
6	151.0	150.2	150.2	151.8
7	150.6	150.7	152.1	152.0
8	150.5	149.9	153.7	150.9
9	148.7	149.9	146.8	149.8
10*	150.5	148.9	148.8	148.5
11	147.6	148.8	150.0	148.4
12	148.2	148.2	146.3	148.5
13	148.9	149.3	149.1	148.0
14	150.7	149.4	148.6	146.8
15*	148.5	149.2	142.7	144.7

* Trial given with increased motivation. † Three point moving average.

It was *E*'s aim to keep at a minimum the variable influence of such factors. For consistent results, it is necessary not only that *E* be skilled in the use of the apparatus, particularly in the rapid reading and recording of the time data, but also that he be adept in the management of the *S*'s and in the use of methods for maintaining a high level of attention and effort. Even in such an elementary motor function as is involved here, the rôle of psychological factors in the test situation must receive consideration, both in the testing and in the interpretation of the results.

Table I presents mean scores in milliseconds (ms.), both sexes combined, for each trial in the 1936 testing. Standard deviations for the successive trials range from 22.5 to 39.1. From the first trial to the fifteenth, a gain of approximately one-half a standard deviation may be noted. It is significant that this adaptation factor is as great for the left hand as for the right, although the left is tested after fifteen trials have already been taken by the right hand. Moreover, as a result of

four previous testings the *S*'s are thoroughly familiar with the apparatus and procedure. Scores for trials 5, 10, and 15, which were given with increased motivation, are indicated by asterisks. The effect of this added incentive on trials 5 and 10 is not readily discriminable from the general warming-up effect, suggesting that a close-to-maximum motivation was maintained throughout. The mean values obtained on final trials compare fairly closely with the results obtained in earlier studies; Ladd and Woodworth quote results for eight investigators, whose determinations for reaction-time to an acoustic stimulus average 146 ms.[3]

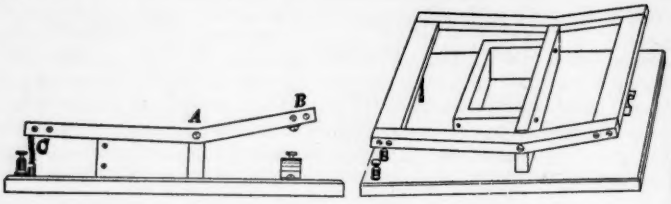


Fig. 2. Variable Tension Reaction-Key

No systematic differences can be observed between the two hands. To avoid confusion in administering the test with younger children, trials for each hand were given consecutively rather than in alternation, and hence there is a possibility that the left hand (tested after the right) received some transferred adaptation-effect. The results obtained, however, are similar to those reported by Goodenough, who found no right-left difference even when trials for the two hands were alternated.[4]

The measures obtained by this type of standardized testing have been described by Miles as representing a "natural reaction-time,"[5] in contrast to the physiological time approximated after extended practice.

Subsidiary Experiment

The effect of practice and of the type of motor reaction, have been investigated in a subsidiary experiment.

Apparatus. Fig. 2 shows in side and top view a reaction-key constructed for use with springs of varying degrees of tension. It consists of a mahogany frame hinged at *A* to per-

[3] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 476 f.
[4] F. L. Goodenough, The development of the reactive process from early childhood to maturity, *J. Exper. Psychol.*, 18, 1935, 431–450.
[5] Miles, *op. cit.*, 385.

mit free up and down movement. When depressed by pushing down at *B*, a contact is established. A series of small coil springs were attached, in successive experiments, at *C*. Four spring tensions were used. (1) Tension 1 required a very light pressure of the forefinger to maintain the contact, and was close to the minimum necessary for abruptly breaking the contact when the finger was removed. (2) Tension 2 required noticeable effort when opposed by one finger. (3) Tension 3 was too great to be successfully opposed by one finger, and required the use of four fingers on the reaction bar. (4) Tension 4 required the use of the whole hand.

Procedure. Two procedures were used in these experiments: in a 'free' procedure, *S* was directed to hold the frame down with his forefinger, and at the sound of the chronoscope motor to lift his finger as quickly as possible. All the *S*'s tended to react not merely by lifting the finger, but by some movement of the hand or forearm as well. In a 'controlled' procedure, *S* was directed to keep his hand on a hand rest, and to limit range of movement as much as possible to the fingers.

Subjects. The *S*'s tested were 5 boys and 6 girls, with an age range of from 9.5–11.9 yrs. and a mean age of 10.9 yr.[6] On 4 successive days every *S* was given 5 trials by each of 6 methods. The re-set device was not used in this experiment and hence scores were not reported to *S* after each trial.

Results. The results of the subsidiary experiment are given in Table II which shows the mean reaction-times in milliseconds of all the *S*'s for the different procedures, for the different tensions, and for the different days.

TABLE II

MEAN REACTION-TIME (IN MS.) AS A FUNCTION OF PRACTICE AND OF THE TYPE OF MOTOR REACTION

Procedure	Tension	Day 1	Day 2	Day 3	Day 4	Mean
Free	1	198.3	190.8	192.5	191.6	193.3
Controlled	1	203.3	196.6	189.2	204.2	198.3
Controlled	2	216.7	204.2	194.2	213.3	207.1
Controlled	3	201.7	193.3	194.2	182.5	192.9
Controlled	4	215.0	215.9	208.3	195.8	208.8
Free	1	202.5	194.2	195.1	200.8	198.2
Mean		206.1	199.2	195.6	198.0	199.8

Comparison of the results of Days 1 and 2 indicates some probability of a slight practice effect. The controlled procedure, limiting the muscle groups involved in the overt response, appears to have no advantage over a relatively free procedure, at this age-level. The amount of spring tension appears to be a factor deserving consideration; in the forefinger tests, an increase in tension results consistently in slightly longer reaction-times. The maximum tension, requiring a total hand pressure, apparently produces tonus effects which to a slight degree impede rapid response. Fully reliable differences, however, cannot be established from this experiment; the most striking fact is the close similarity of the results obtained under different testing conditions.

PRINCIPAL EXPERIMENT

Reliability. In the principal study, after preliminary testing in 1932, the tests were administered in a standard form to approximately one-

[6] The writer is indebted to Miss L. S. Cobb for assistance in this experiment.

half of the sample in the spring of 1933, and to the total sample (excluding closed cases) in the fall of 1933, 1934, 1935, and 1936 (see Table III).

TABLE III

NUMBER OF CASES TESTED, WITH DATA CONCERNING CHRONOLOGICAL AGE

S		Spring 1933	Fall 1933	Fall 1934	Fall 1935	Fall 1936
Boys	Number	52	98	94	89	82
	Age range	2.46	2.54	2.66	2.73	2.80
	Mean age	12.05	12.43	13.49	14.48	15.44
Girls	Number	58	100	88	83	81
	Age range	2.88	2.92	2.82	2.82	2.60
	Mean age	11.86	12.40	13.45	14.45	15.52

TABLE IV

ODD-EVEN RELIABILITY COEFFICIENTS
(Corrected for length of test by the Spearman Brown formula)

	Fall 1934		Fall 1936	
	Boys	Girls	Boys	Girls
Right hand	.864	.887	.855	.900
Left hand	.890	.889	.829	.882

Table IV shows the odd-even reliability for two representative years. It is apparent that 15 trials are adequate for a fairly reliable measure of the reaction-time; an increase to 30 trials for each hand would be desirable if the testing could be done in two cycles with intervening rest periods. This was not feasible in the present experiment, in which the reaction-time tests constituted the first part of an extensive series of motor ability tests.

Table V presents correlations for the right and left hand to be compared with the odd-even coefficients of Table IV. Corrected for attenuation, these right-left correlations are all above 0.92, but are still, for the most part, low enough to indicate a substantial degree of specificity for identical functions involving bilaterally symmetrical effectors. This suggests that for reaction-time (as well as for strength and motor coördination) significant individual differences exist in dextrality ratios, *i.e.* in the relative proficiency of the two hands.

TABLE V

CORRELATION OF MEDIAN REACTION-TIMES FOR RIGHT AND LEFT HAND

	Fall 1934	Fall 1936
Boys	.803 ± .025	.855 ± .021
Girls	.832 ± .021	.828 ± .026

Retest correlations computed for the right hand only, are shown in Table VI. The P.E.'s range from 0.03 to 0.05. Corrected for attenuation, the coefficients representing a year's interval range from ap-

proximately 0.7 to 0.8; those for longer intervals are slightly lower. Although the correlations in 5 out of 6 comparisons are slightly higher for boys than for girls, the sex differences are too small to be reliable. Retest coefficients have also been computed for the left hand, with results closely similar to those shown in Table VI.

TABLE VI

RETEST CORRELATIONS
(Right hand)

	Boys			Girls		
	Fall 34	Fall 35	Fall 36	Fall 34	Fall 35	Fall 36
Fall 33	.716	.589	.572	.600	.584	.548
Fall 34		.707	.630		.671	.644
Fall 35			.722			.695

TABLE VII

REACTION-TIME: MEANS AND S. D. BY GRADE AND SEX

Date	Grade	Boys					Girls				
		Mean age	Right hand		Left hand		Mean age	Right hand		Left hand	
			Mean	S.D.	Mean	S.D.		Mean	S.D.	Mean	S.D.
Spring 1933	H6, L7	12.05	169.2	18.8	167.7	18.7	11.86	170.0	25.5	169.0	20.8
Fall 1933	7	12.43	154.2	20.2	152.0	17.6	12.40	155.3	23.2	157.8	22.3
Fall 1934	8	13.49	156.3	19.2	154.5	20.6	13.45	155.7	19.0	156.7	19.5
Fall 1935	9	14.48	145.7	16.2	143.4	16.9	14.45	147.9	20.3	146.8	19.2
Fall 1936	10	15.44	151.6	16.4	149.6	17.7	15.52	148.5	18.7	151.2	17.7

Age differences. Table VII presents in ms. a comparison of means for boys and girls, for each of five testings. A significant gain is shown in the 6-mo. period from the spring to the fall of 1933. Subsequent years show only slight and fluctuating changes. Because of the possibility that age-gains may be masked by the age-spread at each testing, a more homogeneous subsample was chosen, consisting of cases falling within two months of the mean C.A. of the main group (see Table III). Fifteen boys and 20 girls were found, each of whom had been tested 5 times from the spring of 1933 to the fall of 1936. Table VIII gives the seriatim scores for this group. The boys in this sample show no improvement in score after the age of 12.4 yr., but an apparent gain is exhibited by the girls.

One of the handicaps involved in a growth study, somewhat offsetting the advantage due to comparability of sample, is the factor of adaptation which may be introduced by cumulative testing. Age gains cannot readily be discriminated from practice gains, unless a control group is used. Such a group was established for the present study, consisting (in 1936) of 50 pupils of the 10th grade matched with a representative selection of the principal adolescent sample; the matching was on the basis of grade, age, and sex; 25 boys and 25 girls were included. For this control group, not previously tested, a mean reaction-time of 162.4 ms. with an S.D. of 13.9 was obtained for the boys, and a mean of 162.2 ms. with an S.D. of 22.3 for the girls (right hand only). These means are poorer, by about two thirds of an S.D., than the means for our main group of equivalent age in 1936. It therefore seems probable that some influence of adaptation must be considered in interpreting apparent age trends in this material.

TABLE VIII

REACTION-TIME MEANS IN SUCCESSIVE TESTINGS FOR 15 BOYS AND 20 GIRLS

	Boys		Girls	
Date	Mean	S.D.	Mean	S.D.
Spring 1933	173	16	189	38
Fall 1933	152	18	156	28
Fall 1934	155	21	157	20
Fall 1935	152	18	151	18
Fall 1936	152	18	143	19

Reaction-time changes in the adolescent period have been reported by Koga and Morant from very extensive data collected by Galton (reaction-time to sound, as measured by the Galton pendulum chronograph).[7] The mean score at 11.5 yr. was 169 ms., with a downward trend to 156 ms. at 16.5 yr. Philip has also reported systematic age-changes in this period in reaction-time to sound;[8] the smoothed mean for boys at 9 yr. is 173 ms., decreasing to 140 at 15 yr.; the mean for girls is 181 ms. at 9 yr., decreasing to 150 at 15 yr. Surber, on the other hand, finds no gain among boys after 13 yr., and a negligible gain among girls.[9]

Philip's data, and particularly Galton's, must be scrutinized with reference to a possible selective factor in successive cross-sections, and also with reference to the possibility that motivation is more adequate

[7] Y. Koga and G. M. Morant, On the degree of association between reaction-times in the case of different senses, *Biometrika*, 15, 1923, 346–372.

[8] B. R. Philip, Reaction-times of children, this JOURNAL, 46, 1934, 379–396.

[9] P. Surber, *Reaktionen auf Schallreize*, 1931, 49.

in the upper ages. If this latter is an important factor, a study giving attention to motivation would be expected to show an earlier peak in the age curve.

That this test function is subject to rapid growth in the earlier years of childhood is clearly shown by Goodenough, who reports (for boys) means decreasing from 492 ms. at 3.5 yr. to 218 ms. at 9.5 yr.[10] The corresponding figures for girls are 518 ms. and 202 ms. These tremendous gains are paralleled by the data in Table IX, which gives results in ms. for the initial testing of 5 groups of *S*'s ranging in age from four and a half years to adult age.

TABLE IX

REACTION-TIME AS A FUNCTION OF AGE
(Right hand, both sexes)

Sample	Mean	S.D.
Berkeley Infant Growth Study* (37 cases; age 4.5 yr.)	398.3	78.3
Nursery school 'alumni' (34 cases; age range, 5 yr.; mean age, 7.3 yr.)	250.8	73.3
Adolescent group† (76 cases; age range, 2.3 yr.; mean age, 10.9 yr.)	186.7	25.0
Control group (50 cases; age range, 2.0 yr.; mean age, 14.7 yr.)	162.5	18.3
College students (40 cases; mean age, 19.5 yr.)	156.7	23.3

* Reported by Poe-Eng Yu, in a manuscript on file at the Institute of Child Welfare, Berkeley, from data collected by Dr. Nancy Bayley.

† From initial testing in 1932.

Since Table IX represents different cross sections of cases and several examiners (using, however, the same apparatus) full comparability cannot be claimed for the several groups. The age means are in general lower than those reported by Goodenough, but exhibit the same trend.

Sex differences. Fig. 3 presents (in 1/120 sec.) comparative percentile curves for boys and girls for each of 5 testings. While the right hand curves show no difference with regard to the medians, the girls fairly consistently exhibit a greater variability, with a slightly heavier incidence of both inferior and superior scores. In the case of the left hand records, the boys maintain a small degree of superiority. The sex difference is less than would be expected from the data of Goodenough,[11]

[10] Goodenough, *op. cit.*, 437.
[11] Goodenough, *op. cit.*, 439 f.

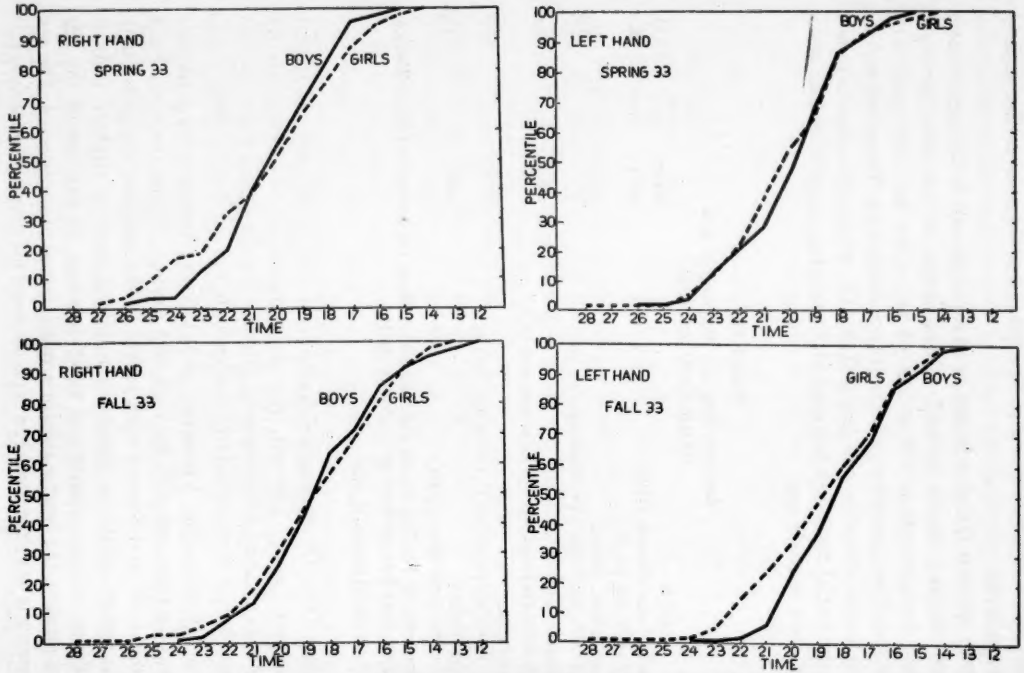


FIG. 3. COMPARATIVE PERCENTILE CURVES FOR EVERY ONE OF THE 5 TEST-PERIODS
(Reaction-Times in 1/120 sec.)

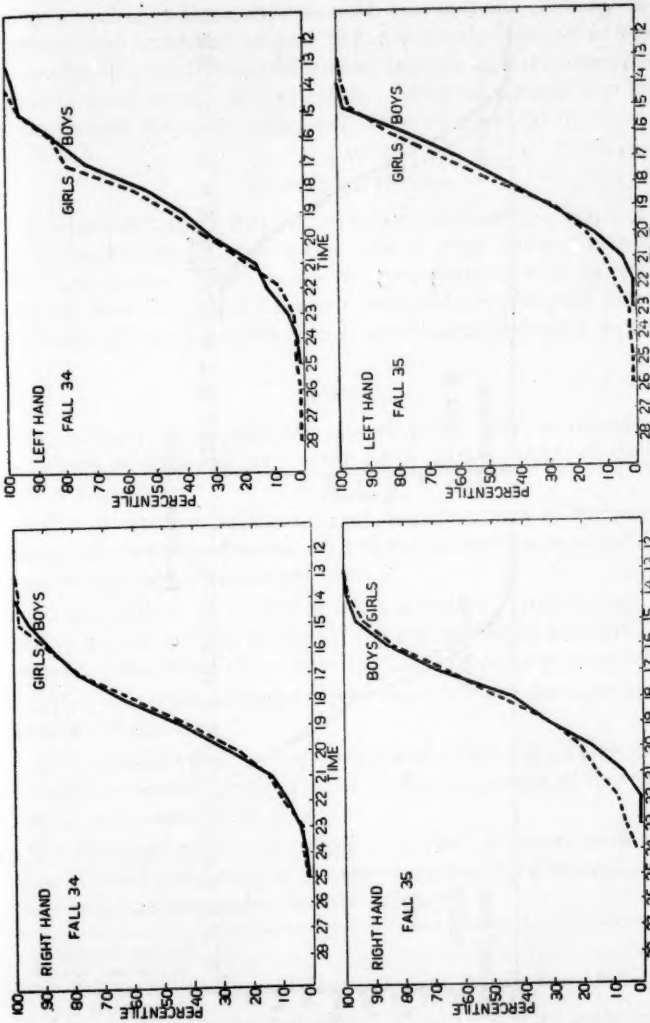


FIG. 3 (Continued)

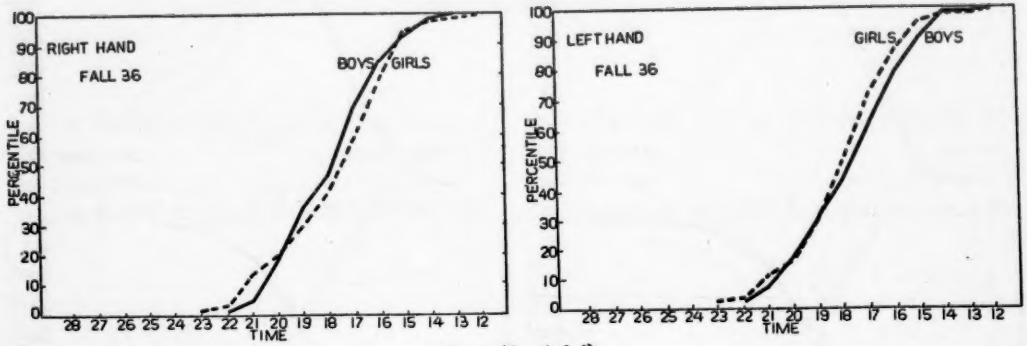


FIG. 3 (Concluded)

Philip,[12] Surber,[13] Bellis,[14] Musselman,[15] and Harmon.[16] It is in general more apparent in the earlier years of the study; its tendency to disappear (with increasing adaptation and under the influence of motivation adjusted to individual differences) suggests that the superiority of boys in reaction-time is a superficial difference dependent primarily upon interest and upon a competitive urge in motor skills.

Studies in Progress

A subsequent report will deal with the relationship of reaction-time at successive ages to performance time in other motor functions, in word-association, and in mental test performance; with its relationship to pulse rate, basal metabolic rate, and physiological maturity; and with the use of reaction-time data in 'characterological' studies.

Summary

(1) As a part of an adolescent growth study, serial measurements of reaction-time to sound were taken for a normal sample of pupils in grades 6 to 10.

(2) A preliminary study indicated the importance of motivational factors; 'knowledge of results' after each trial was the principal device used to maintain attention and effort.

(3) Comparison of successive trials indicated a warming-up effect which was particularly marked in the first five trials, but which continued to some extent throughout the fifteen trials given to each hand.

(4) No systematic differences were found between the right and left hand in reaction-time.

(5) A subsidiary practice experiment with 11-yr. old children showed a slight (unreliable) practice effect in the comparison of Day 2 with Day 1, but no subsequent gains.

(6) An experimental comparison of a 'free' procedure with a procedure in which the range of movement was restricted showed a slight (unreliable) advantage for the 'free' method.

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[12] Philip, *op. cit.*, 388 f.

[13] Surber, *op. cit.*, 49.

[14] C. J. Bellis, Reaction-time and chronological age, *Proc. Soc. Exper. Biol. & Med.*, 30, 1933, 801–803.

[15] J. R. Musselman, On the correlation of head measurements and mental agility in women, *Biometrika*, 18, 1926, 195–206.

[16] G. E. Harmon, On the degree of relationship between head measurements and reaction-time to sight and sound, *ibid.*, 18, 1926, 207–220.

(7) Consistently longer reaction-times were obtained when the spring tension in *S*'s key was increased; higher tensions (requiring heavier pressure on the key) are unfavorable to rapid release.

(8) In a study of reliability and constancy, odd-even correlations for 15 trials (with Spearman-Brown correction) ranged from 0.83–0.90; right-left correlations from 0.80–0.86; retest correlations after a year's interval from 0.60–0.72, and after three years' interval from 0.55–0.57.

(9) In an analysis of age differences in the main sample, in a subsample homogeneous as to age, and in several additional samples of younger children, marked age gains were shown from 4.5 yr. through the preadolescent period, with a slowing rate of improvement in early adolescence and only slight gains after 12.5 yr. A control experiment indicated that a part of the apparent age gain in the adolescent period must be attributed to adaption factors.

(10) An analysis of sex differences showed a slight tendency for the males to be superior; this is more marked for the left than for the right hand, and more marked in earlier than in later years of adolescence.

SOME BASIC CONCEPTS IN PARENT-CHILD RELATIONSHIPS

By Percival M. Symonds, Teachers College, Columbia University
Editor, *The Journal of Educational Psychology*
(Founded in 1910)

Within recent years a new vocabulary has been brought into use in the field of child guidance, including such words as *rejection*, *over-protection*, *aggression*, *hostility*, *over-ambition*, *over-indulgence*, *dominance*, *submission*, which has considerable significance for the education of personality. These concepts certainly grow out of psycho-analysis and the Freudian ideology, although it is very difficult to find in Freud's writings these very terms or even in cleancut form the concepts on which they are based.

Freud's thinking is not in behavioristic terms but in mentalistic terms. For him experiences in the family lead to such outcomes as *anxiety*, *love* and *hate*. The following quotations stating Freud's position may be seen to be the forerunner of recent analyses in more behavioristic terms.[1]

> Fear of castration is not the only motive for repression. . . . In its place is found fear of the loss of love, obviously a continuation of the fear of the infant at the breast when it misses its mother. . . . If the mother is absent or has withdrawn her love from the child, it can no longer be certain that its needs will be satisfied, and may be exposed to the most painful feelings of tension . . . every stage of development has its own particular conditions for anxiety. The danger of mental helplessness corresponds to the stage of early immaturity of the ego; the danger of loss of object or of love corresponds to the dependence of the early years of childhood; the danger of castration to the phallic phase; and finally, fear of the super-ego. . . . As development proceeds the old conditions for anxiety should vanish. . . . But this only happens to a very incomplete degree. A great many people cannot overcome the fear of loss of love; they never become independent enough of the love of other people, and continue their infantile behavior in this respect.
>
> Infantile sex-life perished in most painful circumstances and with feelings of a deeply distressing nature. Loss and failure in the sphere of affections left behind on the ego-feeling marks of injury comparable to a narcissistic scar which, according to my experience . . . yields the most important contribution to the 'inferiority complex' common among neurotics.[2]

[1] S. Freud, New introductory lectures on psychoanalysis, *Int. Psychoanal. Lib.*, 1933, (no. 24), 115 f.
[2] Freud, Beyond the pleasure principle, *ibid.*, 1922, (no. 4), 20 f.

The undesirable result of 'spoiling' the small child is to increase the importance of the danger of losing the object (the object being a protection against every situation of helplessness) in comparison with every other danger. It therefore encourages the individual to remain in the state of childhood, the period of life which is characterized by motor and mental helplessness.[3]

Of course too much parental tenderness becomes harmful because it accelerates the sexual maturity, and also because it 'spoils' the child and makes it unfit to temporarily renounce love or be satisfied with a smaller amount of love in later life. One of the surest premonitions of later nervousness is the fact that the child shows itself insatiable in its demands for parental tenderness; on the other hand, neuropathic parents, who usually display a boundless tenderness, often with their caressing awaken in the child a disposition for neurotic diseases. This example at least shows that neuropathic parents have nearer ways than inheritance by which they can transfer their disturbances to their children.[4]

Healy, Bronner, and Bowers refer, without giving specific reference, to many illusions in psychoanalytic literature of the importance of parent-child relationships. For instance, they say,

Freud calls attention to cases in which the mother is affectionally or sexually unsatisfied by her husband and makes her child her love-object, pouring out boundless tendernesses and caresses upon it.[5]

These terms begin to emerge somewhat more clearly in the writings of such psychoanalysts as E. Jones and J. C. Flugel. Flugel very clearly recognizes the significance of parent-child relationships and foreshadows the emphasis to be given to these relations later on.

Parents who are too severe, too expressive, or even too careful, as regards the upbringing of their children, will—especially if the latter happen to possess strong tendencies to self-assertion—often bring about a state of revolt against their own authority, in which all that may be good and wise in that authority is deliberately neglected or condemned, since the children have grown to look upon their parents as tyrants and taskmasters rather than as helpers and protectors. A stern or bullying father, a nagging or overanxious mother, will thus frequently produce a rebellious son or daughter, who will respect neither the advice or commands of the parents themselves nor those of their (mental) substitutes in later life.

A high degree of initiative, self-confidence or masterfulness in the predominating parent may easily cause the child—unless himself endowed with these characteristics to the same or to an even greater degree—to abandon himself habitually to the supremacy and initiative of the parent and thus in time to develop a lack of those qualities which distinguish the personality of the latter. Curiously, a lack of energy or authority in the parent may compel a child to fall back constantly upon his own power of decision and resource, thus developing in him, to some degree at least, these character qualities in which his parents were defective.[6]

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[3] Freud, Inhibitions, symptoms and anxiety, *ibid.*, 1936, (no. 28), 162 f.

[4] Freud, Three contributions to the theory of sex, *Nerv. & Men. Dis. Monog. Ser.*, 1918, (no. 7), 83.

[5] W. Healy, A. F. Bronner, and A. M. Bowers, *The Structure and Meaning of Psychoanalysis*, 1930, 184.

[6] J. C. Flugel, The psycho-analytic study of the family, *Int. Psychoanal. Lib.*, 1921, (no. 3), 46.

Actually these concepts and terms seem to have precipitated out of the child guidance movement which had its beginnings in 1921 with the stimulus given by the Joint Committee on Methods of Preventing Delinquency set up by the Commonwealth Fund. It is not until about 1930 that these terms are used in clinical discussions in the published literature.

Kenworthy,[7] writing in 1926, made no use of these terms although the ideas underlying the concepts are present in her paper, but in 1929 she makes free use of the term *rejection* and is apparently groping for *over-protection* by her use of such terms as *over-dependence*, *over-solicitousness*, *over-devotion*, and *over-fostering*.

So it may be seen that the use of these terms is of recent development. These concepts represent an analysis of parental behavior which has a functional relation to child behavior and hence are destined to aid measurably in our understanding of the factors influencing child behavior and the development of personality. Not only can these terms be used to describe parent-child relations, but they apply equally well to teacher-pupil, master-servant relations, and, in fact, practically any form of human relationship. And therein lies their importance, for they are concepts which are of importance in understanding the development of personality in any human relationship.

To David Levy,[8] a psychiatrist attached to the Institute of Child Guidance in New York City, belongs the credit for bringing these concepts out into clear relief. That these concepts were brewing for some time before they received explicit expression may be seen in the informality of the first published reports to mention them.

Levy's hypotheses as to the origin of maternal over-protection and rejection were given experimental study through the examination of case material by a series of studies carried on at the Smith College School for Social Work under the direction of Helen L. Witmer.[9]

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[7] M. E. Kenworthy, Psychoanalytic concepts in mental hygiene, *Family*, 7, 1926, 213-223.

[8] David Levy gives his analysis of maternal over-protection in A method of integrating physical and psychiatric examination, with special studies of body interest, over-protection, response to growth and sex differences, *Amer. J. Psychiat.*, 9, 1929, 169 f. He also discussed the same topic in a Second colloquium on personality investigation held under the joint auspices of the American Psychiatric Association, Committee on Relations of Psychiatry and the Social Sciences and the Social Science Research Council, November 29, 30, 1929, *ibid.*, 9, 1930, 879-1027; and again in a paper on Maternal over-protection and rejection, *Arch. Neur. & Psychiat.*, 25, 1931, 886-889.

[9] H. L. Witmer, Studies in maternal over-protection and rejection: I. Introduction, *Smith College Stud. Soc. Work*, 2, 1932, 181-187; E. Hough, II. Some factors in the etiology of maternal over-protection, *idem*, 188-208; P. Foley, III. Early responsibility and affect

After reviewing the mass of literature on this subject and attempting to reduce the confusion of thought to some sort of orderly and significant basis it seemed apparent that there are *two* main factors which are at work. One of these is the *rejection–over-protection* factor; the other, the *dominance–submission* factor. Flugel recognizes these two factors dimly without separating them clearly. In one chapter he deals with the love and hate aspects of development and in another the dependence aspects. Kenworthy also sees the same two factors independently at work. She says:

> Two broad trends of expression stand out clear'y. First, the period of dependency for the child is a prolonged one because for many months he is completely cared for by the parent through every kind of service, and even in the later years until he has achieved the status of being grown up he still requires a continuation of some of the dependency patterns though he has every interest and desire to become independent. . . . The relation of the parent to the child in the rôle of an authoritative person furnishes the second broad trend of each child's experience. . . . The authoritative emphasis which the parent utilizes or the lack of it, is dependent upon many personal as well as environmental elements.[10]

Both of these factors may be thought of as existing in amount or degree. *Rejection–over-protection*, for instance, may be conceived as a continuum and it is possible to describe parental behavior lying at different points along this continuum or scale. Fitz-Simons,[11] as a matter of fact, has constructed such a scale, with statements running from +4 (Parent indulges child—cannot refuse requests) through 0 to −4 (Parent uses severe punishment).

The *dominance–submission* of parents may also be thought of as lying along a scale in varying amounts. Dominance-submission scales have been thus constructed, although not so as to apply particularly to parental behavior.

The behavior of any parent may then be thought of as occupying a point or a two dimensional surface. For instance, a parent might rate +4 in over-protection and −4 in submission. Such a parent might be characterized as over-indulgent, and his position on a surface could be reported by the cross (×) as follows:

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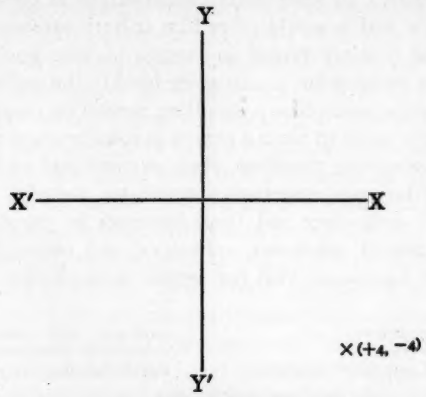
hunger as selective criteria in maternal over-protection, *idem*, 209-223; M. Lewenberg, IV. Marital disharmony as a factor in the etiology of maternal over-protection, *idem*, 224-236; M. Figge, V. Some factors in the etiology of maternal rejection, *idem*, 237-260; C. Brunk, VI. The effects of maternal over-protection on the early development and habits of children, *idem*, 261-273; M. Freeman, VII. Factors associated with length of breast-feeding, *idem*, 274-282.

[10] P. R. Lee and M. E. Kenworthy, *Mental Hygiene and Social Work*, 1929, 30.

[11] M. J. Fitz-Simons, Some parent-child relationships as shown in clinical case studies, *Teach. Coll. Contr. Educ.*, 1935, (no. 643), 1-173.

X coördinate = over-protection—rejection; Y coördinate = dominant—submissive. Names could be given to various extremes of parental behavior as follows: neglect (−4, −4); cruelty (−4, +4); over-protection (+4, +4); indulgence (+4, −4).

The point of origin represents the ideal parent-child relationship. Such a parent neither over-protects nor rejects his child—he gives affection moderately. He provides the necessary environment and care



for a child's growth and protection, but does not so care for and coddle him as to prevent him maturing emotionally and gaining independence. Such a parent is neither too strict and severe nor too lenient.

Any parent on the X axis represents different degrees of rejection–over-protection while maintaining an even and moderate degree of dominance. Any parent on the Y axis represents different degrees of dominance or submission while keeping evenly balanced between rejection on the one hand and over-protection on the other.

The matter is complicated, however, by the lack of clearcut attitudes or behavior on the part of parents. On the one hand parents may be inconsistently rejecting or overprotecting. On the feeling side this is called "ambivalence" and the term may be borrowed to designate inconsistent rejecting–over-protecting behavior. A familiar example of this is the parent who rejects the child fundamentally, but through a sense of guilt compensates for this rejection by over-protection. In such cases the ambivalence may be noted by badly placed over-protection. In such cases a child may be heaped with toys or clothes or may have superficial whims catered to, but neglected in some more

fundamental ways. Or vice-versa a mother may give a child excellent care as regards health or education but may deny the child certain pleasures or childish desires. A parent may also be inconsistent in his dominant-submissive behavior to the child, being severe and punitive at one time and lax and neglectful at another.

It should be recognized that a parent who is *ambivalent* or *inconsistent* does not occupy a *single* point on the surface used to plot these two scales. Indeed an ambivalent parent might at different times exhibit behavior which would place him at both extremes of the scale. In such a case it is not proper to average his two positions and give him a median ranking but to recognize frankly the ambivalence. Likewise inconsistent disciplinary handling should be recognized as such and no attempt made to place a person in some average position.

One may recognize, therefore, three extreme and undesirable forms of the rejection-overprotection continuum; namely, *rejection*, *overprotection* and *ambivalence* and three extremes in the dominance-submission continuum: *dominance*, *submission*, and *inconsistency*. The first three may be associated with the second three in nine different ways as follows:

dominant—rejection
submissive—rejection
inconsistent authority—rejection

dominant—over-protection
submissive—over-protection
inconsistent authority—over-protection

dominant—ambivalence
submissive—ambivalence
inconsistent authority—ambivalence

If, in addition to the above classifications, it be seen that it makes a difference which parent is aggressive or rejecting, the number of possible combinations is increased to 81. If still further it may be admitted as an hypothesis that it makes a difference whether it is a boy or a girl to whom the parental attitudes are addressed the number of possible combinations increase to 162. I shall not go on to estimate the combinations to include possible sibling relationships—whether siblings are treated alike or differently by parents—but such differences are certainly of importance and would increase many-fold the possible combinations. If, in addition, one considers all possible combinations of broken homes, presence of grandparents or other relatives living in the home, and the influence of comrades, teachers, persons in the community, it may be seen that in the possible varieties and combinations of these two factors in parents and others there is a basis for explaining some of the many variations of personality in children providing it can be demonstrated that these factors have any influence at all.

An important point to be decided is whether these terms refer to feeling or behavior on the part of parents. They *may* refer to either. A parent may hate a child or love a child and he may also behave toward a child as though he hates or loves him. Maslow has evidence that feeling and behavior are very loosely related as regards aggressiveness.[12] A person may feel decidedly aggressive or hostile toward another without necessarily giving his feeling overt expression and vice-versa, although the two are related in actuality to a low degree. It is generally believed that any feeling is bound to express itself in behavior sooner or later and to a less or greater degree. However, unless the feeling does express itself in behavior, it obviously can have no effect on the child toward whom the feeling is directed. Behavior in this sense as the expression of feeling does not have to be strongly overt but may be subtle and elusive. It may be nothing more than a shrug of the shoulder, the lifting of any eyebrow, or the gleam of the eye; it may be a chance remark expressing fondness or hatred; it may be the mere fact that things are done together by parent or child, or the child is not included in the parent's plans and activities. These subtle and elusive tokens of rejection or over-protection are just as significant from the point of view of parent-child relationships as the more overt forms of expression. In this paper such terms as rejection, over-protection and the like refer to behavior, although in many cases the behavior is paralleled by feeling.[13]

The questions concerning parent-child relationships as described in the foregoing analysis reduce themselves to three: (*1*) What (parental) behavior characterizes the extremes on the two continuums *rejection–over-protection* and *agressiveness–submission*? (*2*) What child behavior is related to each combination of these two behavior tendencies in parents? (*3*) What in the background of either parent or in the relationships between the parents is related to parent behavior toward a child? In the following pages the literature relating to these questions will be reviewed.

(*1*) Attempts to define rejecting behavior are scarce. It will hardly serve our purpose to use Witmer's definition "the rejecting mother is one whose behavior toward her child indicates a strong dislike for

[12] A. H. Maslow, Dominant feeling, behavior and status in women, paper read at Eastern Branch of the American Psychological Association on April 3, 1937.

[13] Marian Fitz-Simons in her "Guide for the Estimation of Parental Attitudes" has a section of "Key Points" to take care of the feeling of the parent for the child. When one looks at the items in this section it is seen that they refer to just as real behavior as in the main section. Fitz-Simons finds it necessary to include this special list of "feelings" entitled Key Points to take care of the ambivalent over-protection. The analysis given in this paper recognizes that parental *behavior* in such cases is inconsistent and contradictory.

him."[14] Newell, in his first study,[15] used as the criterion for the selection of rejected children a definite statement by the mother to the effect that the birth of the child was unwelcome but this is equally unsatisfactory. Newell's later definition which includes the following ten items of parental behavior is more satisfactory: "severe punishment, neglect, nagging, indifference, irritation, mother threatening to send patient away, handling patient inconsistently, suspiciousness, resisting spending money on patient, and comparing patient unfavorably with a sibling."[16] One will note that the behavior in this list is both aggressive and submissive. The following list of behavior indicating rejection taken from Fitz-Simon's study is still more adequate.

Behavior indicating rejection on the part of a parent.

Parent sees mostly shortcomings
Parent uses severe punishments
Parent deserts child
Parent evicts child
Parent turns child over to an authority
Parent puts child in institution, reform, boarding school, convent (for discipline or to avoid trouble)
Parent does not provide financial support
Parent criticizes child
Parent threatens to evict child
Parent deliberately frightens child
Parent locks child away—in closet, basement, outside house
Parent threatens to report child to authority
Parent pays no attention to child—gives no money, toys, treats
Parent spanks child, whips child
Parent does not provide advantages, special education
Parent does not spend time with child
Parent neglects child—movements unsupervised
Compares child unfavorably
Parent is hard on child—holds to standard unsuited

Over-protection has been much more completely defined due to Levy's interest in and analysis of this form of parental behavior.[17] Levy sets up four categories to define over-protection.

(*a*) *Excessive contact of mother with child.* For many years the mother may sleep with the child, regardless of his sex. She may fondle him unduly and may try to keep him always within her sight.

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[14] H. L. Witmer, Studies in maternal over-protection and rejection, *op. cit.*, 2, 1932, 181–187.
[15] H. W. Newell, The psycho-dynamics of maternal rejection, *Amer. J. Orthopsychiat.*, 4, 1934, 357–401.
[16] Newell, A further study of maternal rejection, *ibid.*, 6, 1936, 576–589.
[17] Cf. Levy, *op. cit.*, *Arch. Neur. & Psychiat.*, 25, 1931, 886–889.

(*b*) *Prolongation of infantile care.* The mother may feed the child by breast or bottle for an unusually long period, or she may dress and bathe him long after he is able to care for himself.

(*c*) *Prevention of development of independent behavior.* The mother may fight the child's battles for him, may supervise too closely the choice of his activities, and may defend him from all criticism from the outside.

(*d*) *Lack or excess of maternal control.* She may over-indulge him in regard to privileges and possessions, may wait upon him, and allow him to upset completely the home routine. Or, on the other hand, she may insist on complete obedience so that she overmasters his infantile traits and produces the child who is "too good."

Hattwick,[18] in her study of the interrelations of preschool behavior and factors in the home, characterized over-attentiveness in the home as follows:

(*a*) *Parent favors child.* Parent "gives in to the child whether he is right or wrong." Parent "lets child have his own way."

(*b*) *Parent is over-solicitous.* Parent "expresses concern over trifles, is over anxious."

(*c*) *Parent treats child as baby.* Parent "doesn't let child do things he could do for himself." Parent "makes unnecessary choices and decisions for the child."

(*d*) *Household revolves around child.* "Child is given first consideration in everything." "Everything at home is planned with the child in mind."

The following list is derived from Fitz-Simons' study to be descriptive of behavior which is over-protective:

Behavior indicating over-protection on the part of a parent.

Parent indulges child—cannot refuse requests
Parent cares for child's physical needs to an unusual degree
Parent sleeps in same bed with child
Parent spends all possible time with child
Parent pushes child
Parent gives money, toys, special privileges
Parent does not want child to leave for recreation
Parent spends time with child—playing with or amusing child
Parent gives in to child
Parent excuses child—defends him
Parent is proud of child—praises him
Parent sleeps in same room
Parent sees few if any faults
Parent protects child from unhappiness
Parent protects child from what he considers harmful
Parent makes an effort to improve child's condition—by carrying out recommendations, etc.

Aggressive and submissive behavior is somewhat more clearly understood. Both aggressive and submissive behavior have been included in

[18] B. W. Hattwick, Interrelations between the preschool child's behavior and certain factors in the home, *Child Devel.*, 7, 1936, 200–226.

the foregoing definitions of rejecting and over-protective behavior. At the risk of some repetition items have been culled out of Fitz-Simons' work illustrating the aggressive and submissive behavior in the following two lists.

Aggressive-compulsive-dominating-hostile behavior on the part of a parent.

Parent 'pushes' child
Parent threatens punishment
Parent nags child
Parent spanks, whips child
Parent is hard on child—holds him to standards which are not suited
Parent criticizes child
Parent threatens to evict child
Parent deliberately frightens child
Parent uses severe punishments
Parent turns child over to an authority
Parent plans for child
Parent cares for child's needs to an unusual degree
Parent gives toys, special privileges

Submissive-neglectful behavior on the part of a parent.

Parent indulges child—cannot refuse requests
Parent gives in to child
Parent does not provide financial support
Parent deserts child
Parent neglects child
Parent does not spend time with child
Parent pays no attention to child—gives no money, toys, treats

(2) The second question raised was: What child behavior is related to each combination of these two behavior tendencies in parents? It would be surprising if this had not been studied inasmuch as it has been open for observation in all generations in human history and is actually a matter of intense human interest. Actually parent-child relations have been systematically studied only within the past few years.

All the various papers and studies mentioned in the footnotes of this article have been carefully reviewed. From this mass of opinion and survey analysis it is difficult to draw more than hypotheses for future investigation. Some of the statements and conclusions are actually contradictory. Fitz-Simons' very careful study yields conclusions which are diametrically opposed to most of the testimony of child guidance experts. In general the statements and studies fail to recognize distinctions which might be made in parental behavior or in child behavior. Few of the writers recognize the possibility that relationships might vary for different combinations of father and mother behavior and attitude as acting on boys and girls independently.

In general the statements and studies indicate that if either or both parents *reject* a child that child is likely to be characterized as: aggressive, rebellious, hostile, jealous, attention-getting, hyperactive, or annoying in school. The rejected child may show such delinquencies as *truancy, thieving,* and finds satisfaction in having his mother upset about him. The rejected child tends to show the patterns known as *emotional instability.* In general the rejected child shows *defense mechanisms.*

On the other hand if either or both parents overprotect a child that child is likely to be characterized as: over-dependent, infantile, possessing feelings of inferiority, withdrawing, poor social adjustment, poor work habits, having good vocabulary, or being poor in arithmetic. These may all be classed as *escape mechanisms.*

In addition to these general relationships Levy states that when maternal overprotection is primarily dominating, submissive traits result—obedience, authority-acceptance, dependence on others—and in boys, effeminacy. When maternal overprotection is primarily indulgent in character aggressive traits result—'authority-reception,' commanding, bullying, and 'limelight' behavior. This latter is so much like the pattern that one expects to follow from parental rejection that one wonders whether indulgent maternal over-protection is not actually disguised rejection.

The pattern, father aggressive and hostile, mother over-protective and sibling rivalry, is mentioned by Levy, Aichhorn and others as leading to pronounced aggression on the part of boys leading to rebellion against society and delinquency.

The pattern, mother aggressive, hostile and rejecting, and father submissive, is reported by MacDonald to be found related in six cases to boys who were passive and effeminate, except when thrust into masculine situations when they would make uncalled for physical attacks accompanied by cruelty, and they show such behavior difficulties as petty stealing, lying, truancy, and sex misconduct.

There is some indication that the mother's behavior and attitude are considerably more potent in forming the personality of offspring than the father's behavior and attitude.

Watson found that homes in which the training was *strict* and *severe* tend to have children who "hate their parents, quarrel with associates, are unable to live on a mature and independent basis, are socially maladjusted, full of over-conscientiousness, guilt, and fears, are sickly and definitely unhappy." This would seem to be an over-generalization

inasmuch as it is not the kind of home that probably makes the difference but the specific relation of parents and the individual child.

Hattwick found that "homes which reflected signs of tension in the form of frequent illness, fatigue, impatience, quarrelsomeness or nervousness made for uncooperative behavior and poor emotional adjustments."

These generalizations are most provocative. Evidently there are relations in this area but the studies which have been made up to the present time, having failed to define and isolate the variables, give results which are vague and uncertain.

(3) The third question: "What in the background of either parent or in the relationship between the parents is related to parent behavior toward a child?" has also received considerable discussion and testimony from clinical experience, but little from statistical survey and none from experimentation.

These studies are based on a too small number of cases and doubtful statistical procedure for the conclusions to have much generalized validity. In general it may be said that both parents who reject their children and parents who are over-protective have had unfortunate and unsatisfactory relations with their own parents in their youth, and this relationship is further complicated by poor marital relationships.

In the case of rejection psychoanalysts report that hostility to a child is a direct transference of the unconscious hostility to a parent of the same sex as the child.

In the case of over-protection the same factors are recognized to be responsible: an unhappy childhood and frustrations in marriage. In addition the over-protective mother sometimes is responding to greater hazards for survival in her child than is usual, or is continuing with her own child tendencies toward responsibility and dominance for others which she has been forced to adopt in her childhood toward her brothers and sisters.

A PSYCHOLOGICAL CYCLE

By Samuel W. Fernberger, University of Pennsylvania
Editor, *Journal of Experimental Psychology*
(Founded in 1916)

A survey of the history of the development of psychological technique and of psychological point of view indicates that there are fashions of psychological thought exactly as there are fashions in clothes or in social custom. And a parallel may be drawn further between these several sorts of fashions. In all of these cases a new fashion is all too frequently extreme and overemphasized. But, if the fashion lasts, the good becomes evident and it assumes its place with its proper proportion among the already existing fashions.

In recent years in psychology in America there has been a fashion to magnify 'objective' techniques and to look askance at any technique which might be considered to have a subjective basis. It seems to me that the fashion is beginning to change and that some of the 'subjective' techniques are again beginning to attain importance in current psychological methodology. Let me hasten to say that I have no quarrel with objective techniques nor with some of the magnificent results which have been obtained by them. The present paper is an attempt to reconcile some of the differences between the proponents of the two different points of view in the hope that psychologists will realize that all of us are working toward the same ends and that we are all working in the same scientific universe.

Just a year ago I began to consider writing this paper. At the meeting of the Eastern Branch of the American Psychological Association in 1937, there was a round table discussion on introspective techniques. The surprising thing about that session was that something over 70 persons were present, and that most of them stayed for the entire session which was a rather long one. Many of those then present contributed to the discussion. And still more surprising was the fact that most of the participants were young—so young that they had certainly not reached psychological maturity until the Watsonian Era was well established. At first I suspected that many of these young people had discovered a new word—introspection—on the program and that they

had come to find out its meaning. But such was certainly not the case. Indeed many had the general background of the problem which I would now outline very briefly.

There is no need of telling you of the importance of the introspective method for psychology in the past. In 1901 the *Baldwin Dictionary*[1] in discussing "introspective method" says in part. "The introspective method is essential to psychology, and it has been followed by psychological writers in all ages from Plato and Aristotle downward. The main advantage of the method of experiment in psychology is that it gives opportunity for introspection under test conditions." Thus the method of introspection, 35 years ago, was considered still the primary and fundamental method of psychology. But, in fairness one must note that the definition goes on to say: "But no psychologist ever confined his procedure to mere introspection. It was always more or less supplemented by inference from resulting products to producing processes, and by observation of the manifestations of mental processes in other minds." Or in other words, psychologists utilized their own introspective results to interpret, by analogy, what was going on in other minds. And this interpretation of behavior was applied to animal as well as to the human mind. You will find that the experimental literature of the earlier days abounds with such analogies in the interpretation of animal behavior. Animals did all sorts of things which were described in subjective terms—they were "shy," "angry," "excited," "puzzled," and what not besides.

I need not bore you with a detailed description of the development of introspective techniques. Under Titchener's direction at the Cornell Laboratory and elsewhere, the introspective method developed technical procedures, a highly technical terminology and recognized dangers against which the observer must be cautioned. Professor Baird, at Clark University, also contributed largely to the development of these techniques. Then the pronouncements of the Würzburg Imageless Thought movement under Professor Külpe created a controversy out of which grew still finer technical distinctions. For example, *Beschreibung* was differentiated from *Kundgabe*—psychological description of process as against information about the data of experience. And the American introspectionists almost always said "about the *mere* data of experience.' "

If one may translate this development of the interest of psychologists in introspection into the form of a curve, he will find that it begins with Plato and Aristotle, rising slowly but steadily until the time of

[1] J. M. Baldwin, (Editor) *Dictionary of Philosophy and Psychology*, 1, 1901, 568.

John Locke. Then, with the British Empiricists, it received added momentum which would be evidenced by a much more rapid rise in the curve. And, with the beginning of the experimental period in psychology, still further momentum was added. It is safe to say that the curve of the use of the introspective method reached its climax in the second decade of the present century.

Of course there have been criticisms of the method. It would be a waste of time to more than mention the philosophical criticisms that here one had a dual observation—observation of the process and observation of the observer—or of the philosophical inability to prove the existance of a mind other than one's own. The modern and youthful psychologist may know that this literature exists but I wonder how many of my younger listeners have read extensively in it.

There were other criticisms of a more personal nature frequently dependent upon a particular personality and frequently based on a particular incident. In this connection, I wish to tell a story which I think is worth recording. Professor Witmer went to Leipzig to study with Wundt in 1891.[2] He was asked to act as subject in an experiment in progress which involved the recording of sensory reaction-times. It happened that Witmer's reaction-times were considerably shorter than those of any other subject in this experiment or, indeed, of any which had previously been obtained in other experiments in Wundt's laboratory. The experimenter accused Witmer of giving the more rapid reaction of the motor type rather than the sensory type called for by the experimental conditions. On the basis of his own introspective evidence, this Witmer denied. In spite of the warning, he continued to give reactions which were so short as to be out of line with the then accepted standards. So Witmer was called in to Wundt's office and told that he must be giving motor reactions, that he should make a cardboard pendulum and train himself in giving reactions of the slower sensory type and that, if he did not reform, he would be dropped as a subject from the experiment. Witmer denied again, on the basis of his introspective evidence, that he was not entirely conforming to the conditions of the experiment. His reaction-times remained short and he was promptly dropped as a subject. It will be realized that in 1891 the wide range of individual variability in reaction-times was not known. And anyone who knows Professor Witmer even slightly will realize that he is an extreme "hair trigger" type of individual.

[2] This story comes directly from Professor Lightner Witmer and I have recently checked the facts recorded with his memory of the events.

The result of this incident was that Witmer was definitely turned against introspection and its use as a psychological method. The effect of this on me personally is also worth recording, in all probability. In my undergraduate and graduate work at the University of Pennsylvania, under Witmer's direction, I can hardly remember the word introspection being used and certainly I can never remember any experiment which called for the use of the introspective method. Immediately after I received my degree, I was appointed an Instructor at Clark University. Here, under the direction of Professor Baird, were being performed some of the most remarkable introspective studies to be found in the literature—Fisher's work on abstraction and generalizing; Woods on recognizing; Wheeler on volitional choice.[3] I was asked to act as observer in all of these experiments. I wish now to report that, at the end of two weeks, I went to Professor Baird and told him that I did not know really what they were talking about; that I was either a total failure in psychology or, at least, was not at all properly prepared for this job and that it would be best if I resigned. Wise, as always, Baird calmed me down, steered me judiciously to some reading and all was well.

But the principle criticisms against the introspective method in those days were leveled, perhaps, at the technical difficulties developed for its use primarily by Titchener. *Kundgabe* must be avoided and, if it was impossible to completely avoid a statement of knowledge about process, then one must place this *Kundgabe* in parentheses in one's introspective report—and thus mark it off from, and apparently keep it from contaminating, the true *Beschreibung*. The concept of the "stimulus error" was invented and the psychologist was warned that he must observe process and describe it in psychological terms completely without reference to stimulus or anything else.

If I may be allowed to indulge in reminiscence for one last time, I should like to tell of an event which stands out in my memory as something of a nightmare. In 1914, Baird was asked by the Program Committee of the American Psychological Association to give a public demonstration of his use of the introspective method. In what was most certainly an unwise moment, he consented. The room was crowded. Dr. Fisher showed new material to a group of observers—material similar to that she had used in her study of abstraction and generaliz-

[3] S. C. Fisher, The process of generalizing abstraction, and its product, the general concept, *Psychol. Monog.*, 21, 1916, (no. 90), 1-213; E. L. Woods, An experimental analysis of the process of recognizing, this JOURNAL, 26, 1915, 313-387; R. H. Wheeler, *An Experimental Investigation of the Process of Choosing*, 1920, 1-59.

ation. After exposure, each observer was asked to define the figure, by description, and then to give an introspective account of the mental processes concerned with the development of this definition. My memory is that it was a good show. I was an observer and at least Professors Woods and Wheeler gave introspective reports. Looking back on the experience it was an unreasonable task to give any observer. The emotional strain of such publicity is not condusive to fine detailed introspective report.

It is needless to say that the demonstration fell flat. And the *coup de grace* was delivered by Watson who said substantially: "It has certainly been a waste of the spectators' time to have put on this show. I would have expected an averagely bright High School student to have described the figures better than any of these alleged experts." In this statement, Watson was both right and wrong. The High School students probably could have given a better description of the figures. But Watson missed entirely the point that we were not particularly interested in describing the figure and that our problem was the description of the mental processes by which we arrived at that definition. The demonstration was ill advised because those familiar with introspective techniques as practiced at that time did not need it and all the other psychologists could not be expected to understand what was happening or to appreciate its value.

To return to my cyclic curve, one may say definitely that, by this time, there was a strong downward trend in the importance of introspective techniques for psychology. The cautions and the intricacies of the methods developed since the beginning of the century were probably largely responsible for this decline but also responsible was the development of new objective techniques over the same period. Introspection, to be accepted by the best introspectionists, became too cumbersome to be handled and there was another outlet for the activities of the research psychologist. This made the time ripe for the advent of Behaviorism and its formulation in the able hands of Watson. Rapidly the curve of the interest in introspective techniques descended to an all time low by the end of the second decade of this century. By that time, anyone who mentioned the validity of introspection was looked upon with suspicion of being stupid, queer or, at least, hopelessly old fashioned. True, it was pointed out that Watson admitted "verbal report" as an acceptable method of behavioristic psychology but one would have been laughed at had he attempted to give a Titchenerian type of introspection as a Watsonian verbal report.

Nevertheless much had been accomplished by introspective techniques during the first 20 years of the century. During this time it had been established that all of the so-called higher mental processes were capable of analysis into a small number of constituent elements and principles of explanation. This was so well established that it will probably never have to be repeated—even though the Gestalt psychologists have subsequently pointed out that such an analytic approach was wrong in the first place. Baird was responsible for the genetic approach toward any particular process and the results from his laboratory have indicated beyond question that each particular mental process changes considerably during development and by habituation.[4] These results emphasized the difficulty or even the impossibility of attempting an introspective description of a highly mechanized and therefore syncopated and foreshortened mental process. The Würzburgers added the concept of intention and the determining tendency and, although their method was criticized by the Titchenerian school, the results were accepted and verified and today form the nucleus of some of the most important problems in the psychology of thinking.

By 1920 introspection was certainly at a low ebb. But "Truth crushed to earth will rise again." The cyclic curve did not remain at the bottom for long. Soon Gestalt psychologists were insisting upon introspection as a valid and important method of psychology—but a changed introspection as compared with the American Model. In place of this, they suggested phenomenology—by no means new in the history of psychology. I cannot do better than to quote a short section beautifully told by Koffka in his recent book.[5] "For us phenomenology means as naïve and full a description of direct experience as possible. In America, the word 'Introspection' is the only one used for what we mean, but this word has also a very different meaning in that it refers to a special kind of such description; namely, the one which analyses direct experience into sensation or attributes, or some other systematic but not experimental ultimates. . . . This kind of introspection became unpopular in America because American psychologists saw its barrenness. But in their justified criticism they threw out the baby with the bath, substituting pure achievement experiments and tending to leave out phenomenology altogether."

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[4] In this connection, *cf.* Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 117), 1-161.

[5] K. Koffka, *Principles of Gestalt Psychology*, 1935, 73.

Words such as these fell on fertile ground because we were never without psychologists who were practicing the introspective method—the curve never reached zero. I venture to say that there were never many psychologists who completely and whole-heartedly accepted behaviorism, that is, who contented themselves with a thoroughgoing description of behavioristic reaction without some interpretation involving actual or implied introspectively obtained concepts. For most psychological problems, the complete behavioristic approach has proven as barren as the Gestalt psychologists have accused the American type of introspection of being.

The reaction from Behaviorism and the criticism of Gestalt psychology have both had their effect and, it would seem, that the cyclic curve of the use of the method of introspective report is definitely and, one may judge, rather rapidly moving upward. If you will turn from one to another of the recent psychological texts, you will find the change is everywhere evident. When writing a textbook of psychology, no psychologist today is hesitant about adding chapters on the image, on thinking or other subjective material. Turn to the short but highly significant preface of the 4th Edition of *The Animal Mind* by Professor Washburn[6] and you will find this statement: "The principal change in the attitude of investigators of animal behavior since the third edition of this work appeared is the decay of behaviorism as an interpretation and the revival of animal psychology. The conclusions drawn from experiments are now expressed in subjective terms."

But this revival of introspective technique and of interpretation in introspective terms has led to a certain confusion which does not seem to be entirely terminological. In a recent widely read psychological text appears the following: "The term for this observation of experience is introspection. The data that introspection shows are called phenomena, and the totality of them is phenomenal experience. Phenomena are such as memories, thoughts, dreams, perceptions, sensations. Illusions always point to phenomena with special clarity because in them the lack of correspondence between phenomenon and its stimulus is notable. The straight stick (stimulus) looks bent (phenomenon) in water. The steady room (stimulus) spins around (phenomenon) for the dizzy man. The common pedestrian (stimulus) looks sinister (phenomenon) to the paranoid patient."[7]

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[6] M. F. Washburn, *The Animal Mind*, 4th ed. 1936, v.

[7] E. G. Boring, H. S. Langfeld, and H. P. Weld, *Psychology: A Factual Textbook*, 1935, 4. Each chapter of this text was written by a different hand and, in every case, the name of

It seems to me that part of the trouble here is terminological because the word "introspection" has developed its own special meaning and the word "phenomenological" has its own and very different meaning. One may include both in a single definition as Boring, Langfeld, and Weld have done and succeed in selling the definition to the beginning student who has no historical background. For the beginning student this is probably the best procedure. But certainly, a confustion will exist in the mind of the psychologist whose background gives the different special meaning to these two terms. What we need is another term which will include both introspection and phenomenology and, let me hasten to say, that I shall not suggest this term in the present paper for two good reasons. In the first place I have not been able to think of a suitable or adequate term and, secondly, I hesitate to invent a new word and so further enlarge our already full and cumbersome psychological terminology.

And it seems to me that none of the recent attempts to redefine introspection have, or are likely to become, widely accepted. For example, the recent definition by Tolman reads: "In introspection an organism behaves both to his own behavior-adjustments (q.v.) and to a listener. Introspective behavior expresses a sign-gestalt-expectation (q.v.) in which the goal object (or significate) will be the immediate conjunction between the introspector's own behavior-adjustments and the listener, and in which the sign-object is the very complex one consisting of the behavior-adjustments which are thus to be presented to the listener plus the listener, and in which the means-end-relations are a very complicated set involving social, verbal, rhetorical, etc. dimensions (q.v.)."[8] I am unfair to Tolman by lifting this definition out of the total text. He has cut the knot of crystallized meanings by inventing a new terminology. But I shudder to think of trying to teach either this definition or the concepts involved to a beginning college sophomore!

Although I refuse to add to the terminological difficulties by suggesting a new term, it seems worth while to delimit a new conceptual definition of method. As a psychologist, in many psychological experiments, I wish to obtain *information about processes which I cannot observe directly*. For example, I present two weights successively to a human

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the author appears in a footnote. The first chapter, from which this quotation has been taken, carries no such notation of authorship. One must suspect, therefore, that it is from the hands of the three editorial authors jointly. This is probably true because it is a chapter dealing with general background and points of view.

[8] E. C. Tolman, *Purposive Behavior in Animals and Man*, 1932, 447.

subject and I want information regarding his experience. In such a situation, I can get a large number of different kinds of report. The subject may give me an introspection on the lines of the American model and I will obtain knowledge of the processes underlying the formation of his judgment.[9] Or I may obtain a phenomological report from which I shall gain knowledge of the basis and nature of the judgment.[10] Or the subject may give a simple verbal report—the usual procedure in psychophysics—from which I may merely gain knowledge of the judgment itself. And this last type of knowledge may be gained by a large number of different kinds of report besides the more usual verbal form. The observer may give a verbal report, if he is a human, or he may make a prearranged gesture or he may press a particular one of several keys. If the observer is an ape and properly trained, he may place a blue or a white poker chip into the "chimp-o-mat" and receive a bit of food as a reward. If he is a dog with proper training, he may salivate or he may raise his leg. But, in all of these cases, I will obtain knowledge, through report, of some psychological process which I am *unable to observe directly*.

It has been found that the situation is extremely complicated for any of these modes of report. Newhall and Rodnick have shown recently that such factors as speed, facility, practice and the like have an effect upon the response.[11] Here indeed is a complicated problem on which much experimentation still remains to be done. Neet recently reported experiments in which he obtained thresholds of auditory intensity by the methods of verbal report, manual response, and a conditioned respiratory response.[12] He found that the verbal response method yielded thresholds lower than those obtained by either of the other methods. But, on the whole, the thresholds obtained by all three methods are of similar magnitude. In an examination of the results, I am more impressed by the similarities of the final results obtained by the three methods of report than I am by the actual differences.

And the conditioned reflex method turns out to be much more complicated than Pawlow himself at first envisaged it. This Pawlow himself recognized when he was willing to talk about "the Reflex of Pur-

[9] Fernberger, *op. cit.*

[10] Cf., for example, H. Friedländer, Die Wahrnehmung der Schwere, *Zsch. f. Psychol.*, 83, 1910, 129-210; and Fernberger, An experimental study of the "stimulus error," *J. Exper. Psychol.*, 4, 1921, 63-76.

[11] S. M. Newhall and E. H. Rodnick, The influence of the reporting-response upon the report, this JOURNAL, 48, 1936, 316-325.

[12] C. C. Neet, A comparison of verbal, manual and conditioned-response methods in the determination of auditory intensity thresholds, *J. Exper. Psychol.*, 19, 1936, 401-416.

pose" and the "Reflex of Freedom."[13] Razran has shown that the attitude of the observer during salivary conditioning in humans has a great influence on the course of such conditioning.[14] Examples could be multiplied but these are sufficient to indicate that (1) one may obtain similar information from widely different types of response and (2) that there exist subjective factors in even the most 'objective' types of response which must be known and controlled if we are to set up a set of experimental conditions which can be duplicated.

It would seem that introspection, phenomenology, and the various sorts of report—verbal and reactive—have several aspects in common: (a) they give information about processes which are not directly observable; and (b) in every case, the report is always in *symbolic form*. The pressure of a key or a conditioned reflex is quite as symbolic as the use of language. The realization of the symbolic nature of *all* report clears up at least one of the criticisms so often hurled against the use of the introspective method; namely, that the observer must be trained. Surely! He must be trained in the use of a specific symbolic vocabulary. But so must the dog be trained in the symbolic use of the salivary reflex. The real problem is that each of these symbols must be identical in meaning for the observer, for the experimenter, and for him who will read the report of the experiment.

I shall simply call all of this "report" and we may define report as a symbolic reaction (verbal or non-verbal) on the part of the observer which will give knowledge to the experimenter of some psychological process about which the experimenter would otherwise not have information. The method of report, thus, again becomes the fundamental method of psychology. The kind of report which one will employ will depend on the nature of one's problem, on the kind and training of your observer and upon one's experimental conditions. And one can definitize one's procedure, if you wish, by the addition of an adjective before the word 'report' and you may indicate that you employed "introspective report," "phenomenological report," "verbal report," "manual report," "conditioned reflex report," or what not besides. In this way, one may gain psychological knowledge of different kinds depending upon the adequate choice of the method of report, determined by the purpose and the nature of the experiment.

If such a concept as is here outlined seems adequate, then many con-

[13] I. P. Pavlov, *Lectures on Conditioned Reflexes*, 1928, 275-286.
[14] G. H. S. Razran, Attitudinal control of human conditioning, *J. Psychol.*, 2, 1936 327-337.

flicts of point of view may be eliminated and psychological method becomes more unified than it seems to have been in the recent past. Psychologists would again come to realize that they are all dealing with merely different aspects of the same field and that they are all working in the same experimental universe. And this unification is so much to be desired in the present situation when psychologists all too often do not—or pretend that they do not—understand what other psychologists are talking about. And, with the adoption of report as here defined as the fundamental method of psychology, the cyclic curve would again tend strongly upward and, one might believe, rapidly approach a new maximum.

PIONEERING IN APPLIED PSYCHOLOGY AND PERSONNEL

By James P. Porter, Ohio University
Editor, *The Journal of Applied Psychology*
(Founded in 1917)

Students and workers in the fields of Applied Psychology and Personnel are pioneers. Measured in terms of the years required for the development of either a genuine science or art both are in the stages of infancy. Many of us who are engaged in making contributions to this memorial volume should feel closely akin. We should have mutual sympathies, understandings, tolerances, humilities, and challenges. There is something really challenging in the pioneering work of placement, personnel, and psychology, pure I imagine, as well as applied.

Let us first trace some of the changes which have come about in psychology during the past third of a century. Dr. C. M. Louttit, formerly of the Ohio University staff, has made a careful study of the *Psychological Index* computing the percentage of publications falling in the fields of General and Experimental, Nervous System, Abnormal, Applied Psychology, and Personnel. A chart reproduced as Fig. 1 portrays these changes in a very suggestive manner.

It is to be observed that publications using the term Personnel in the title are not found at all until about half or more of the period (1895–1929) has elapsed. Yet by the close of this period the number of publications in personnel has grown to 2% of those in all of psychology. It should be understood throughout this discussion that the percentages are only approximate. There is no attempt here to belittle the amount or quality of original study in personnel or any other branch of psychology. Rather do we see a remarkably rapid development in a very brief time. The effect on workers in personnel and closely allied fields should be an incentive to a greater number of pure and applied investigations.

We observe in passing an interesting and significant change in the curves during and after the war. Even before the United States entered the war the relative changes from year to year were slight. Terman's study in 1921 shows an almost equal division as between pure and

applied psychology.[1] In terms of number of studies listed in the *Index* the proportion is not so very different in 1929. More careful classifica-

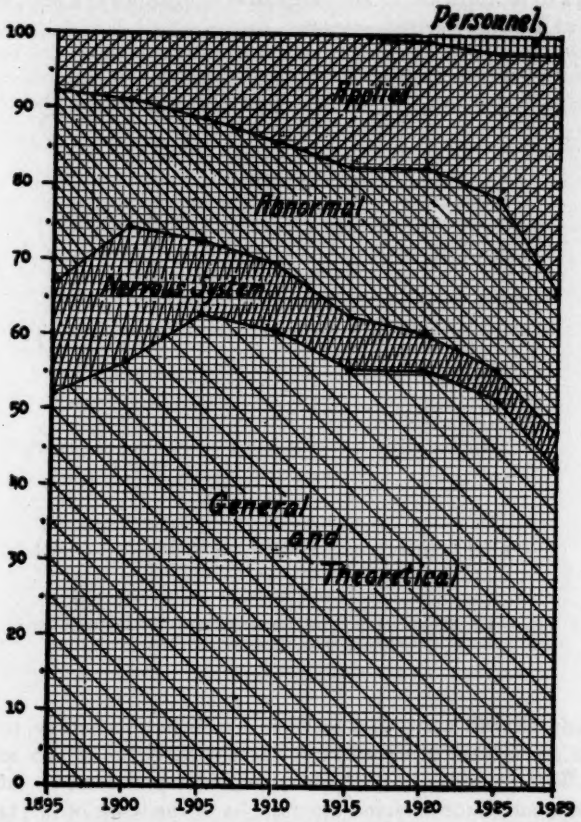

FIG. 1. PERCENTAGE DISTRIBUTION BY MAJOR DIVISIONS OF TITLES IN THE *Psychological Index: 1895–1929*

(After C. M. Louttit)

tion might reveal that applied psychology is engaging the energy and time of the greater number of psychologists.

In 1926 the University of Iowa issued a bulletin devoted to "Psy-

[1] L. M. Terman, The status of applied psychology in the United States, *J. Appl. Psychol.*, 5, 1921, 1–4.

chology and Service." Dean Seashore has given the accompanying diagram (Fig. 2) which with the explanations represents a pioneer attempt to set forth the relationships and obligations of a new, and to many an unacknowledged, science.

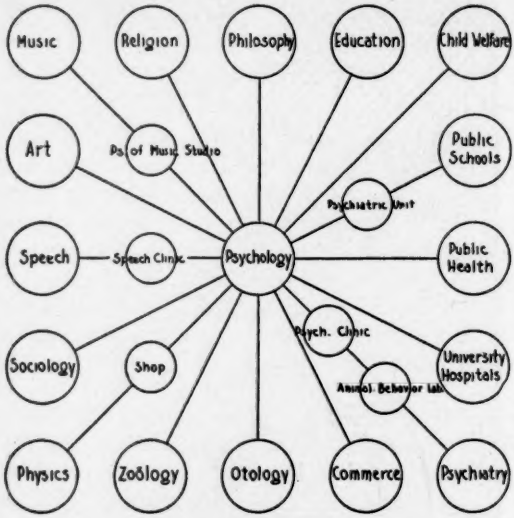

Fig. 2. The Relations of Psychology to other Departments and Services in the University of Iowa

(C. E. Seashore)

Nearly all will agree that Seashore has made psychology at Iowa one of the pioneer departments in the applied fields of this science. Child-Welfare, Music, Art, Speech, and Abnormal come to mind at once as some of the outstanding examples of the work of this laboratory, which is one of the oldest in this country.

Not long ago I exhibited an enlargement of this Iowa chart to a group of university women. Immediately a question arose about the meaning of Otology and the relationships and services which Psychology might reasonably be expected to bear to such another discipline or science. The joint rôles played by each of these in aviation during the war therefore became topics for discussion and at least partial recognition.

A section of this Iowa bulletin reads:

Applied Psychology without a background in pure psychology can lead only to superficiality. Pure psychology is vastly enriched by its sharing of the vision and possibilities of service in applied psychology. . . . . Psychology is peculiarly fortunate in its rich and varied contacts and common interest with other sciences. The University of Iowa has been a pioneer in the development of several of these contacts and now holds national recognition for a thorough-going, coöperative program with the arts and other sciences, in theory and in practice, in instruction and in research.[2]

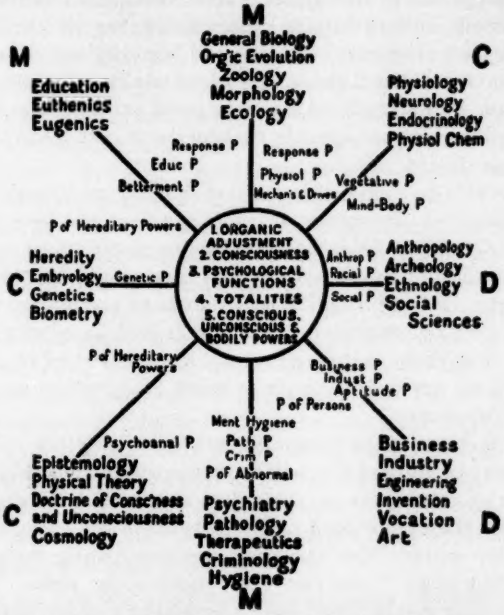

FIG. 3. PSYCHOLOGY'S FAMILY RELATIONS AMONG THE SCIENCES
(Madison Bentley)

It would be difficult indeed to find from any source a statement which would better express the meaning which the writer would like for the title of this present discussion to convey.

The pioneering contributions just indicated become the more interesting when compared with the vice-presidential address, Section I, Anthropology and Psychology, of the American Association for the

[2] Psychology and service: The relations of psychology to other department and services in the University of Iowa, *Bull. Univ. Iowa*, N.S., No. 364, June 25, 1916, p. 3.

Advancement of Science meeting in Cleveland, January 2, 1931.[3] This address also containing a diagram (Fig. 3) was given by Dr. Madison Bentley, then on leave from Cornell University serving as the chairman of Anthropology and Psychology in the National Research Council.

It is obvious that this attempt is more detailed and analytic than its predecessor. Within the circle are phrases of great significance for personnel and placement officers in both business and education. This is particularly true of "Totalities," and "Conscious and Unconscious Bodily Powers," not to mention others. Recent writers in business and industrial psychology, such as Tead and Metcalf,[4] are constantly reminding us that the total personality of the worker must be understood and trained. It is scarcely necessary to point out here that what this really involves is further scientific study of the physiological and mental processes of the individual.

Dr. Bentley's diagram has the merit of showing graphically the kinds of psychology which promise to render the service or bear these relationships. One of the fields most challenging to placement and personnel officers and to applied psychologists is portrayed down and to the right in the chart. This indicates the kinds of psychology related to business, industry, engineering, and art. It will be noted that "P of Persons" is common to this and mental hygiene and psychiatry—two at least of the applications made so much of in modern business and industrial relations.

In this graphic representation provocative suggestions will occur if one explores each one of the radii from the central circle taking note of the kinds of psychology on the radius and in addition the kinds of psychology placed near each radius or between the various radii. As Dr. Bentley writes: "Our outside connections, while many, are not wholly disordered."[5] He also very significantly adds: "For three decades at least psychologists have been reaching out to touch knowledge, the arts, the professions, and the affairs of every-day living. And at the same time all these human interests have been pressing in upon psychology, implicitly confessing that man's learning and man's living require facts and principles not otherwise supplied in the curriculum of the sciences."[6]

Dr. Bentley believes that there are to be found certain distinctive

[3] Madison Bentley, Psychology's family relations among the sciences, *Science*, 73, 1931, (No. 1883), 113–117.
[4] Ordway Tead and H. C. Metcalf, *Personnel Administration*, 1920, 1–538.
[5] Bentley, *op. cit.*, 114.
[6] *Idem.*

attitudes in these relations toward psychology. In general biology, education, and medicine the attitude is a willingness to conduct and care for. He represents this assumed relationship by "M"— the "managing relatives." My own experience and reflection would not lead to the selection of the same three managing relatives. Indeed, I am not certain that I should select more than two. Such selection would probably turn largely on the personnel with whom one has had to labor and coöperate. It is true, however, that frequently when called upon for help the psychologist finds that his well-meant efforts to be of assistance have furnished only another occasion for being misunderstood, managed, and, one might almost say, exploited.

Three other groups sustain a contributory relationship to psychology. These are designated by "C." To Dr. Bentley's way of thinking without the help from physiology, body history, and reflective theory, psychology as it is at present could hardly exist. Two of the groups designated by "D" he states are the chief dependent relatives of psychology. In this group perhaps more than in the others his efforts will be looked upon as those of a pioneer. He very probably would be the first to plead guilty to pioneering. Such analytic efforts are unquestionably of great value. There are signs of progress in the differences in detailed comparisons and evaluations in Dr. Bentley's as contrasted with Dean Seashore's diagram. No doubt the purposes of the two were somewhat different. Nevertheless until it is proved otherwise I shall believe that the study of pure or applied psychology, or both, during the period of time elapsing between the construction of the two charts has helped to clarify certain psychological problems. Foremost among these are the very problems with which personnel and placement officers are directly concerned.

During the past few years with several groups of students an interesting and I believe valuable task has been to formulate the current psychological theories and to see their influences on present-day problems both theoretical and practical. Many students raise questions which indicate a keen interest in the theoretical and practical bearings of the theories here listed. Their later personnel relations work would, I believe, be made much more intelligible and useful could they be given an understanding of the development and relationships of at least these more important theories. Such works as *Psychologies of 1930* and *The History of Psychology in Autobiography*, both edited by Dr. Murchison, and Wheeler's *The Science of Psychology* have been invaluable

sources. Table I contains only a partial list of all of those theories which might have been made.

TABLE I

CURRENT PSYCHOLOGICAL THEORIES

1. Hormic (Purposive) Psychology	McDougall
2. Association Psychology	Brett
3. Functional Psychology	Carr
4. Motor Psychology	Washburn
5. Eclectic Psychology	Boring
6. Structural Psychology	Nafe
7. Gestalt Psychology	Kohler, Koffka, Wheeler
8. Conditioned Response	Pavlov
9. Reflexology	Bekhterev
10. Dialectic Materialism	Kornilov
11. Anthroponomy	Hunter
12. Behaviorism	Watson, Weiss
13. Response Psychology	Dunlap
14. Dynamic Psychology	Woodworth
15. The G and S Theories	Spearman
16. Analytical Psychology	Janet
17. Psychoanalysis	Freud
18. Individual Psychology	Adler
19. Organismic Psychology	Kantor
20. Realistic-Energic Psychology	Porter
21. Naturalistic Psychology	Jastrow

The chief uses which I wish to make of this list of current theories are to show: first, that they are pretty certainly of a pioneering nature, and secondly, that certain ones of them are at least of interest to personnel workers and applied psychologists. It is scarcely necessary for me to add that it will be no occasion for surprise if some of those put down here as exponents of certain theories object to being so classified as well as to the name given the theory for which they are said to stand.

Of all persons those concerned with personnel must at least be interested in hormic (purposive) psychology. They are facing constantly the problems of the aims and motives of the individual. Eclectic psychology has appealed to me in the strongest manner for a long time. It refuses to be tied up to any aspect or theory of mental life. It fails to

offer, however, any definite principle of selection and organization of the items of human mental experience. Gestalt with its emphasis on "totalities" (wholes, configurations) should prove helpful to those charged with dealing with the integration of individual employees and patients. Psychoanalysis, however individual and subjective in its methods of interpretation, is significant for being the first to stress the emotions as determining factors in behavior. Indeed, beginning with the conditioned response and including reflexology, dialectic materialism, anthroponomy, behaviorism, response and perhaps dynamic psychology have major emphases which contributed either to our knowledge of the learning process or to the drives behind it, possibly both. The organismic and naturalistic (term my own) are again suggestive in their attempts to understand the manifold and changing organizations of ideas and responses in mental life.

In order to leave no doubt as to whether or not this discussion is pioneering in nature I am setting down in this list my own theory. As more than hinted at before placement and personnel officers must deal with that which is real. In many places in modern psychology one finds the emphasis on the energies of men. Let us hasten to add that an energy theory is by no means necessarily materialistic. For some years now the best definition of psychology which I could formulate reads as follows: "A scientific study of those accumulative changes of mental energy which adjust man to environmental situations and determine successful or unsuccessful self-development." It will be clear, of course, that it is in part for purposes of discussion that this theory is here set down.

A new and rapidly developing field of study is characterized by intense activity and therefore by strong emotion. Pioneers live intensely. They easily become controversial over differences which they tend to exaggerate. It remained for Dashiell to demonstrate that there were many more resemblances among three fundamental learning theories than their ardent supporters had led us to believe.[7]

He examined typical experimental studies cited by the exponents of these three schools—the conditioned-response, the trial-and-error, and the Gestalt—to find the following fundamental principles common to all: (1) the subject must be motivated; (2) a field of motive exists; (3) obstruction is offered to the principal motive; (4) hyperactivity is aroused; (5) the response is multiple and varied; (6) it is from relations

[7] J. F. Dashiell, A survey and synthesis of learning theories, *Psychol. Bull.*, 32, 1935, 261–275.

to stimuli; (7) most importantly it is between means and objective (8) selection or least action appears; (9) selected reactions occur fortuitously; (10) effects are crucial; and (11) rate of learning varies only in degree.

It is true that Dashiell's three theories of learning are not identical with any one or any single group of the theories in our list. There is, however, so close a relationship as to suggest that too much had been made of apparent differences between these so-called theories.

Then there is that true pioneer in industrial democracy and the more human side of personnel work—Whiting Williams. Again in 1930 he disguised himself and passed for one of the jobless. He found in the American worker "more mobility and resourcefulness, more lightness on his mental and economic toes, his leaders and employers are now much more intelligent and socialized than ever before. Therefore, America should endeavor through longer planning of new products and new markets, not simply to palliate unemployment, but also to prevent it."[8]

This inimitable student of the minds of domestic and foreign laborers finds much to encourage him in 1931 as compared with what he found by the same methods of observation in 1919. His books on *What's on The Worker's Mind*, *Full Up and Fed Up*, *Horny Hands and Hampered Elbows*, and *Mainsprings of Men* together with numerous magazine and journal articles have made an indelible impression on the minds of many persons in positions of leadership in industry and business. Some six months after Whiting Williams purposely disguised himself in order to get first-hand knowledge in business psychology the writer was having a very similar experience and largely with the same purposes. My manager told me about Whiting Williams during a discussion of some of my own findings concerning human relations in industry.

Some of the change of attitude, we hope for the better, is due to the work just described plus the original studies in applied psychology and personnel research. Dr. Estabrooks, psychologist in charge of the Personnel Office of Colgate University, recently stated that, in his experience, representatives of business and industry who employ 75% of Colgate's graduates are little interested in scholarship records.[9] They want men with the personality traits which promise success and therefore profit. The definite task and emphasis of the Arts Colleges must be,

[8] Whiting Williams, The worker's mind today, *Person. J.*, 9, 1931, 401–406.

[9] G. H. Estabrooks, Personality training and the changing emphasis in the liberal arts college, *J. Appl. Psychol.*, 15, 1931, 96–99.

therefore, the conscious training of young men in that integration of personality to meet this demand of the business world.

There are clearly other trends in the changes now going on in the colleges and universities. One of these is the direct emphasis on scholarship by the management of such companies as the American Telephone and Telegraph Corporation. Another is the use of a rating scale on the student by instructors who have occasion to know him best and a follow-up rating on the same scale by the superintendent or supervisor who have to judge as accurately as possible of his actual achievements. Miss Lillian Barbour, one-time secretary of the College Personnel Association and graduate student in Ohio University, has made such an investigation. She has succeeded in making clear that the highest correlations are indicative of only a moderate agreement between what the student does in training and in the later actual work of earning a living, for example, teaching.

To those who give high place to productive scholarship it will be of interest to learn that most of the psychological journals published in the United States are unable to publish without delay all the manuscripts submitted. Some are printing additional issues while others are increasing the size of single issues. *The Journal of Applied Psychology* had recently in its files some sixty-five manuscripts for examination with a view to future publication. While we have published some three dozen original articles on the psychology of advertising in the past ten years there are in hand three or four more, some of which develop new formulae for measuring the effectiveness of certain methods of advertising. The situation is less striking but very similar for studies of personality and its relationships and especially the problems associated with the teaching and learning of psychology itself. We confess to a temptation to publish studies of how psychology is facing reality itself out of turn earlier than they would otherwise appear. It is to be hoped that this is setting a good example among the sciences.

The key emphases in placement, personnel, and applied psychology are selection and training. Let me quote from a recent statement of General Squires in The *New York Times:*

> Teaching men to fly, to send messages by wireless, to operate machine guns in the air, to know artillery fire by its bursts in the air, and to travel hundreds of miles by compass, teaching other men to read the enemy's strategy from aerial photographs . . . required a network of flying fields and schools, a large instructional force and a maze of equipment and curricula.
>
> The job was accomplished by strict adherence to the 'victory creed' of our service, which bears repeating in time of peace as well as in time of war. Here it is: "To foster

individual talent, imagination and initiative, to couple with this a high degree of coöperation, and to subject these to a not too minute direction; the whole vitalized by a supreme purpose which serves as a magic key to unlock the upper strata of the energies of men."

Those responsible for all this skill were engaged and interested in pioneering in new fields of human endeavor.

The AMERICAN JOURNAL OF PSYCHOLOGY, now commemorating its fiftieth year, can well afford to reprint a few words from its fifth volume. Dr. Bryan, in 1892, in one of the best examples of a systematically planned and executed experimental study, in which his findings were analyzed and interpreted by the best known statistical methods, wrote, in his introduction, both ably and prophetically, "We have, however, a far more fundamental justification of concrete Psychological research when we view it as—what it is intended to be—a contribution to a long coöperative task."[10]

[10] W. L. Bryan, On the development of voluntary motor ability, this JOURNAL, 5, 1892, 128.

THE RÔLE OF THE PSYCHOLOGICAL CORPORATION IN APPLIED PSYCHOLOGY

By PAUL S. ACHILLES, The Psychological Corporation, New York
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(Founded in 1917)

Almost twenty years ago, Dr. J. McKeen Cattell made the challenging proposal that psychology might be so capitalized as to earn funds for its own advancement. This proposal subsequently took form in the Psychological Corporation, a business organization owned and operated by psychologists, not for private gain, but for "the advancement of psychology and the promotion of the useful applications of psychology." Insofar as the activities and problems of this organization are representative of the development of applied psychology in America, an account of them appears appropriate in the fiftieth anniversary number of this JOURNAL.

One indication of the Corporation's achievements, before discussing policies and services, is the current project for summer internes. During July and August, 1937, seven psychologists received a full two months' course based on the Corporation's case material in market research, and including some instruction and experience in industrial, personnel work and vocational guidance. This training program was under the full time supervision of Dr. John G. Jenkins who has been one of our most active representatives. The members of this group were paid for their time and their travelling expenses. Three of them were already representatives of the Corporation with some experience. The rest were younger men, recent or present graduate students, selected with the aid of local representatives. This training program was prompted by the fact that there is a great dearth of psychologists adequately trained in the field of applied psychology. The advancement of the whole field of psychology has been greatly retarded by this lack.

Claims for the practical uses of psychology are many, but the major problem of the Psychological Corporation is to make practical use of the services of *many psychologists* to mutual advantage. The fact that their professional status, other than as teachers, lacks clear definition and that, as their numbers increase, many seek quite properly either

to supplement their income or to earn their entire living by offering practical services on their own account adds to the complexity of the problem. Its discussion and the development of the Corporation's policies require first a brief review of the Corporation's form of organization and early history.

History and Policies

The Corporation was formed as a business organization, not a membership society, and it obtained its charter in April 1921 under the business laws of the State of New York. This charter grants broad powers, including the right to establish branches, or subsidiary offices, which may also be incorporated, and contains two unique provisions: one, that the American Psychological Association may purchase the Corporation's stock at any time at one hundred dollars per share; and two, that the dividends paid by the Corporation shall be limited to six dollars per share in any one calendar year. Its constitution and by-laws further provide that, beyond this limited return to stockholders and reasonable compensation to those engaged in its work, all earnings of the Corporation shall be used for the improvement of its services and research. Its officers and directors serve as such without salary. Since its founding, its board of twenty directors has consistently been representative of the leading psychologists in the country, and its presidents have been successively: J. McKeen Cattell, W. V. Bingham, R. S. Woodworth, E. L. Thorndike, and at present, A. T. Poffenberger.

Although it has from time to time administered grants from foundations for special projects, the Corporation has never sought nor accepted subsidy for its own support other than the self-sacrificing services of psychologists. The initial capital of ten thousand dollars with which it started business was raised by an issue of one thousand shares of stock of no par value which was sold, for the most part in lots less than ten shares, only to psychologists, at an original valuation of ten dollars per share. In the 'prospectus' announcing the aims of the Corporation and calling for the coöperation of psychologists in its work, Dr. Cattell wrote tersely: "We shall mix our brains with our money to enhance the value of the stock," thus indicating that returns to stockholders would depend largely on their own efforts. The idea apparently appealed to many psychologists at the time, for about one-half of the then scarcely four hundred members of the American Psychological Association subscribed for stock. To provide the balance

of the ten thousand dollars stipulated as the initial capital with which the Corporation would start business, Dr. Cattell purchased personally the remaining shares issued and he still holds a majority of the shares outstanding and serves as Chairman of the Board of Directors.

With its modest funds in hand, the Corporation opened an office in New York City as its headquarters. Perhaps unfortunately, its rôle was first conceived as principally that of proclaiming what psychologists elsewhere could do. Its advent was widely and favorably noted in the press, and the notion that its rôle was chiefly that of a source of information about psychology and a clearing house for psychologists has persisted to the present day in many references to it appearing in psychology textbooks and popular articles. Although doubtfully tenable from a business standpoint, the theory of this rôle was that instead of training and maintaining a *staff* to render psychological services, the Corporation would refer its clients to psychologists at universities, or elsewhere, to have the services performed and then collect a certain percentage of their fees as its income. Furthermore, this theory presumed that stockholders and other psychologists loosely associated with the Corporation might serve as its representatives and use its name to get business. However, such difficult matters as the extent of the Corporation's responsibility for their actions and whether any or all of the business they obtained should be considered theirs, as gained on their own reputation apart from the Corporation, or be considered that of the Corporation, were not clearly determined. In view of the nebulous character of psychological services and the academic status of most psychologists at the time the Corporation was formed, this rôle for its Main Office might almost be described as rushing in where academic psychologists feared, disdained, or were untrained to tread and trying to obtain clients for them to serve when, as, and if they chose at their convenience. At any rate, the Corporation's attempt to play the part for the first nine years of its existence resulted in a large financial deficit and but little achievement in applied psychology which the Corporation could claim as its own. Possibly its advance notices of what psychologists could do were too far ahead of actual performances. More probably, the rôle was not that of an effective business organization.

The disappointing progress of the Psychological Corporation in its earlier years has sometimes embarrassed American psychologists seeking to explain why a somewhat similar organization, the National Institute of Industrial Psychology, which was launched at about the

same time in England, was apparently so much more successful. Thus for example, Laird writes in a recently published book: "Twenty years ago there was not much psychology could do for a firm except to look wise and make shrewd guesses. Today psychology is in a far different status. . . . The Psychological Corporation, 522 Fifth Avenue, New York City, is an American clearing house of psychologists with which it is worth while to communicate for assistance. In England there is the unique and eminently successful National Institute of Industrial Psychology, Aldwych House, London, which puts to shame anything of the kind in America—or anywhere else in the world—and which is a virile, living monument to Dr. Charles S. Myers, in a country where psychology otherwise is pretty far back in the days of used to be."[1] Laird then discusses the paradox "that although English psychologists are in general far behind those in America, yet in the field of industrial psychology they are so far ahead of us that we find it almost painful." He points out that in this country more use has been made of psychology in the fields of education and mental testing than in industry, and offers two reasons for the greater success of the English Institute in the latter field: One, "the sound organizing ability, conservative salesmanship, and basic scientific stamp which Dr. Charles S. Myers gave to the National Institute of Industrial Psychology." Two, that there was a more virgin soil for such work in England because it had not already been cultivated, as in America, by scientifically, if not psychologically, minded management engineers ever since the time of Frederick W. Taylor. "This," he says, "is probably why our Psychological Corporation, organized and guided by as capable a man for such work as can be found anywhere, Dr. J. McKeen Cattell, did not make the notable practical progress of its comparable English National Institute. In short, our psychologists were good enough, but our business and consulting engineers were too good. American firms too were beginning to feel fed up on efficiency, just about the time that their English brethren first had their eyes opened to it. But even so, our psychologists have done much more in industry than is generally known. Much of their work is kept under careful control of secrecy by the firms which retained them, in order to have competitive advantage from it."

Although the foregoing reasons account in part for the relative progress of the two organizations compared, the following factors,

[1] D. A. Laird, *How to Use Psychology in Business*, 1936, i.

which are generally overlooked, are probably of greater significance. First, that the English Institute was organized with, and continues to have, the coöperation, backing and advice of influential business men. Secondly, that although the Institute charges fees for its practical services, it has been heavily subsidized and depends almost entirely for its research work and publications upon membership dues, solicited contributions and bequests, and grants from foundations. Thirdly, and partly due to subsidy, the Institute operates with a specifically trained, full-time, salaried staff in a relatively small territory, whereas the Psychological Corporation, without subsidy, has endeavored to utilize the part-time services of independent and primarily academic psychologists scattered over a whole continent. Thus, while practically all of the work done in the field of industrial psychology in England can properly be called the work of the Institute, little or none of the similar work done by psychologists in this country can be called the work of Psychological Corporation, unless perchance psychologists loosely associated with the Corporation choose to call it so. Whether or not the Corporation might have fared better in its earlier years if modeled more nearly on the lines of the Institute can never be known. In some respects, it is now operating more as the Institute does, but it has managed to do so without subsidy and in the long run this may prove to be an advantage rather than handicap.

As already indicated, the Corporation's mounting deficits demanded, by 1930, some modification of its rôle as a mere publicity agent for academic psychologists. Accordingly, the experiment was tried of letting a few psychologists, who were found willing to do so at their own financial risk, serve as a headquarters *staff* for the Corporation. These psychologists had not only to provide their own office space and equipment, but perforce to offer their own professional services for a living. Thus it naturally came about that there was less proclaiming of what might be done elsewhere and more performing of what could be done at the Corporation's main office. In short, the shift in corporate policy and procedure was from self-effacement to self-support, and the effect was wholesome. Whatever their mistakes, these staff members at least had to learn by doing in the stern school of business reality where red ink supplants subsidies. Many suggestions offered by psychologists regarding desirable Corporation projects could not, and even now cannot, be undertaken without first reckoning the cost and then earning the money to do them, or else asking psychologists to volunteer their services. In consequence, and not without

desirable results for the Corporation both financial and otherwise, the efforts of its staff were first centered on complying with business requirements and developing services to meet the needs and wants of clients rather than the preferences of psychologists.

Quite obviously, however, a single office supported by the personal clientele of a few psychologists associated with it was not, and is not, the final object of the Corporation. Under the circumstances, it merely constituted a necessary first step and its success now presents the possibility of the Corporation's establishing similarly financed offices elsewhere and, even more desirably, of its developing new and broad-scale types of services which require the coöperation of many psychologists whether they are associated with any of its offices or not. If it be argued that such offices of the Corporation will be in competition with other psychologists, or even with each other, the best answer is that a going concern with a good reputation for scientific work is far more likely to create better opportunities and to provide more clients for all properly qualified psychologists in the field of applied psychology than a dead one. Under a capitalistic system, both competition and coöperation are desirable, and business and profits, which are the sources of all subsidies, properly accrue where services and management merit them. This was the path chosen by the founders of the Corporation and, short of awaiting some Utopia, it still appears the preferable one for the Corporation to follow. The most difficult questions involved are those concerning the nature of relationships to be established between psychologists and the Corporation, the extent of its responsibility for their activities, and the services it can best attempt to capitalize. Pertinent to these questions is the following rough count of the present connections of psychologists as listed in the current yearbook of the American Psychological Association:

Connection	No.
Universities and Colleges	1393
Social Service Agencies (correctional institutions, orphan homes, public and private charities, adjustment and employment bureaus, health, child guidance and mental hygiene clinics)	165
Public School Systems	137
Hospitals	101
Government Agencies, Federal, state, or municipal (exclusive of school systems)	75
Private Schools	47
Subsidized Foundations and Membership Associations	31
Courts	23
Prisons	11
Special Projects	9

Connection	No.
Professional Practice	
Independent; including private clinical practice or research	49
Employed by business firms	47
Associated primarily with psychological consulting and research organizations such as the Psychological Corporation	23
Graduate Student	49
No connection given	146
	2306

At first glance, the results of this count appear anything but favorable to an attempt to capitalize the services of psychologists by means of a business organization. The fields of educational, clinical, and 'personal service psychology,' in which the largest numbers of psychologists are engaged, apart from those with university connections, properly belong to psychologists as individuals, either in salaried positions or in private practice on their own account. The Corporation's experience already indicates that any attempt to sponsor activities or to capitalize services in these fields should be strictly confined to the efforts of psychologists serving as *staff members* at its own offices. Only thus can it assume proper responsibility for them. While the Corporation, as represented by these offices, may well continue to refer individual clients from time to time to psychologists elsewhere for such services, it should no more assume responsibility or share fees for them than does a doctor who refers a patient to a presumably reliable doctor elsewhere. As a matter of fact, owing to public misconceptions of scientific psychology, the handling of many mail inquiries and personal interviews at the Corporation's offices is a considerable expense rather than a source of income, but in the absence of any other agency to perform this function, the Corporation must probably continue to do so until such time as reputable professional psychologists can be distinguished from charlatans with a degree of certainty at least comparable to that in the field of medicine. Clearly the first obligations of the Psychological Corporation are those of a *business organization* and its proper functions are not those of a sponsoring or licensing agency for psychologists, nor those of a professional membership society with the problem of determining proper qualifications for membership. Happily there is prospect that licensing will eventually be handled by legislation and professional qualifications by the proposed "American Society of Applied and Professional Psychologists," or whatever name the society may adopt. Meanwhile the Psychological Corporation must continue to operate as a business organization with psychology and psychologists 'as is.'

In recent years, with the growth in numbers and professional specialization of psychologists, it has become increasingly apparent that delimitation of the Corporation's responsibilities is desirable not only in the clinical, but in *all* fields of applied psychology. In its efforts to expand, the Corporation has associated with it, in addition to its stockholders, many other psychologists, so that the total now connected with it in various ways amounts to over four hundred. While this growth in numbers has the advantage of making the Corporation a "nation-wide organization of psychologists" available for services, it has made the problem of effective business operation more difficult. Certainly the original policy of permitting all psychologists associated in any way with the Corporation to serve as its representatives and to use its name in offering any manner of services they choose must be abandoned. This casts no reflection on these psychologists but is in simple accord with the proverb that "everybody's business is nobody's business" and the fact that the Corporation must exercise proper control of its business if it is to succeed. Hence it is now adopting the sound policy of limiting its responsibilitity for professional qualifications, activities, and services in *all fields* of applied psychology to the psychologists serving as staff members at its own offices. Where other psychologists are involved in the conduct of any services or work for which the Corporation has made financial arrangements and is responsible, it offers specific assignments to and makes agreements with these psychologists without assuming any responsibility beyond their competence for these assignments. No doubt this policy presents possibilities for conflict between the interests of the Corporation and those of individual psychologists seeking business on their own account, but these seem unavoidable and must be handled as equitably as possible as they arise. Inasmuch as the status of most psychologists connected with the Corporation, even those serving as its staff, cannot be clearly defined as that of employees, the success of the Corporation depends entirely upon the voluntary acceptance by psychologists of such financial and other arrangements as may be necessary to enable it to render dependable scientific services and to operate effectively as a business organization. Precarious as this may seem from a business standpoint, the Corporation has thus far found such splendid willingness to coöperate and to make personal self-sacrifices on the part of many psychologists that the attainment of its fine objectives appear well within the range of possibility.

The means to these fine objectives are services of economic value,

and those which the Corporation is now endeavoring to render are summarized briefly on its letterhead as follows: "A nation-wide organization of psychologists of recognized scientific standing which conducts consumer surveys on products, advertising, and sales appeals; applies scientific methods to the selection, training and productivity of sales, office, and factory personnel; acts as counsel on the psychological aspects of sales, personnel and public relations policies."

The Psychological Service Center

It will be noted at once that the above statement omits clinical and counseling services to individuals and services to schools although, as previously intimated, these are rendered by properly qualified staff members at the Corporation's offices. It was deemed more appropriate that such services be presented in descriptive circulars and under another letterhead, as the services of a separate Division known as "The Psychological Service Center, Under Auspices of the Psychological Corporation." This is the manner in which they have been successfully handled at the Corporation's Main Office in New York. Out of the staff's own earnings it has recently been possible to provide segregated office space and more equipment for the staff members engaging primarily or from time to time in such work. Thus many facilities are utilized to double advantage and most of the psychologists of doctoral rank at the Corporation's Main Office serve also as staff for this Center, which is under the direction of Dr. Henry C. Link. Dr. Rose G. Anderson, who joined the Center's staff in 1932, has been notably successful, not only in private case work but in building up a further subdivision which she directs and which is known as the Child Guidance and School Service Division. Thus her time is distributed between the office and a number of public and private schools, and further extension of her work will require the addition to the staff of capable assistants in this field. The clientele of the Center is growing and the standards of its work are indicated by the fact that it is among the agencies in New York City selected as suitable for the practical 'interneship' training of graduate students in clinical psychology at Columbia University. In other cities where properly qualified psychologists have the funds to establish similar offices, the success of this Psychological Service Center in New York can probably be duplicated or surpassed but, as previously stated, it seems inadvisable for the Corporation to accept any responsibility for such Centers unless they are immediately associated with, and under the control of, one of its own offices.

Divisions of Service

Chiefly as a matter of convenience and staff commission arrangements, it has become customary at the Corporation's headquarters to consider and to some extent conduct its work under nominally separate Divisions known as the Clinical Division, mentioned above, the Industrial Division, the Marketing Research Division, and the Tests Division. This practice will be followed here although the work and special problems of each Division can be but briefly described.

The common problem of all of these Divisions is not only to render efficient and scientifically acceptable services but to find a market for them. It is scarcely an exaggeration to say that most clients, whether business firms or individuals, want quick or ready-made answers to their problems, while psychologists are chiefly prepared to do research on them. Psychologists properly have faith that the application of scientific methods will provide the best answers, but it is often difficult to persuade clients, who must pay the bills, that the painstaking methods of science are worth their time and cost. Perhaps the most notable service which the Corporation renders is that of closing the gap between popular notions of psychology and the services which psychologists of recognized scientific standing are prepared to render. It is continually developing an appreciation among business firms of the value of a truly scientific attack on many of their problems and, at the same time and equally needed, it is developing among psychologists a better understanding of these problems and their significance for psychology.

As indicated earlier in this article, industrial psychology has advanced from the stage of shrewd guesses, but in this country it must compete with shrewd exponents of scientific management. In general, the usefulness of psychologists in industry has been limited by their own narrow interests in lack of training in cost accounting, industrial engineering, motion and time study, and other tools of scientific management. They must be well versed in these matters if they wish to speak the business man's language, or to find feasible opportunities *in* industrial establishments rather than their own laboratories, for the application of their techniques and the conduct of researches contributory to waste reduction and the workers' welfare. To some extent the present inadequacies of training will probably be met in future by a more practical curriculum for graduate students in industrial psychology. It has been stated, however, in a recent article by Bingham

that studies in the psychology of *management and supervision* are now most needed and are in fact inescapable because recent investigations have shown that "there are factors of attitude, morale and supervisory relationships even more potent than those of illumination, ventilation, rest-periods, nutrition, wage incentives, or individual skills."[2] If this be so, it behooves psychologists to scrutinize quite as carefully as their training and skills their own attitudes and research interests if they are to be useful in industry. Here, for example, are the purposes of industrial research laboratories as outlined by Mees:[3]

(1) Elimination of manufacturing troubles.
(2) Investigation of possible new products.
(3) Development of standard methods of testing or specifications for purchase of raw materials. (*To this, the psychologist may add 'personnel.'*)
(4) Investigation of new industrial propositions, of which the value has not been commercially established.
(5) Investigation of new methods of using products or improving methods of operation for the benefit of the customers of the firm.
(6) Fundamental scientific research having an important bearing on the technique of the subject in which the industrial company is concerned.

The chances for psychologists in industry are proportional to their capacity to fit themselves into this picture.

The Psychological Corporation can probably best contribute in the field of industrial psychology by continuing its present efforts to build up properly trained and competent staffs at its own offices, and at the same time to coöperate with psychologists at universities or elsewhere who are willing to make suitable arrangements with it for any work to be done under the name of the Corporation. Although such coöperation is highly desirable, and, to the extent it can be gained, it places the Corporation in the position of having many capable investigators, the logical expectation as this field develops is that many psychologists will tend either to seek permanent personal employment with business concerns or to do consulting work on their own account. At present there is also the fact to be faced that many business firms prefer to train persons within their own organizations for psychological work than to employ full-fledged but poorly oriented psychologists. In the long run, however, this tendency for executives to read psychological texts and make their own applications may work out favorably for capable psychologists. The more psychology is practiced as an art in personnel management, the better; and the more likely executives will be to

[2] Walter V. Bingham, The future of industrial psychology, *J. Consult. Psychol.*, 1, 1937, 9–11.
[3] C. E. K. Mees, *The Organization of Industrial Scientific Research*, 1920.

realize the need of employing psychologists with the right training and experience to conduct the time-consuming researches essential to improvements in industrial practices. In fact, the experience of the National Institute of Industrial Psychology in England has been that many firms wish to secure members of its staff for positions in their own organizations and there is already evidence that the offices of the Psychological Corporation may undergo the same desirably difficult experience.

Industrial Division

In New York, the work of the Corporation's Industrial Division is ably directed by Dr. Richard S. Schultz with other staff members as co-workers and consultants. Dr. Schultz has had practical industrial experience and has taken special training in motion and time study work. In this connection, mention should be made of a course in motion and time study, dealing also with its psychological aspects, which was successfully conducted for business executives by the Corporation with the active participation of such authorities as Dr. Lillian M. Gilbreth, Prof. David B. Porter, and Dr. Elton Mayo. Research and technical services in the field of industrial psychology have been rendered by the staff of the Corporation's New York office to many firms, among them such well known and progressive organizations as the Metropolitan Life Insurance Company, the Johns-Manville Company, the General Electric Company, and the General Chemical Company. Its work is steadily gaining favorable recognition and it is becoming better prepared to handle important investigations involving the confidence of top management. There is no doubt that in most cities there are many opportunities for work by capable psychologists representing not only the Industrial but other Divisions of the Corporation if mutually satisfactory arrangements with such psychologists can be made. Owing to the limited time and resources of its headquarters staff, the matter of such arrangements is a difficult and as yet but partially solved problem for the Corporation. If it had the financial resources to do so, the most logical course for the Corporation from a business standpoint would be to open offices in other cities at its own expense and operate them through its own employees. Since it cannot do this and, as previously indicated, the policy of allowing any and all psychologists connected with it to act as its authorized representatives is not feasible, its present position with regard to offices operating under its name in other cities is as yet tentative and experimental.

Test Service Division

The Corporation's Tests Division arose naturally from the fact that the rôle of the Corporation's headquarters was first conceived as that of an authoritative source of information about psychology and that such a rôle could not be played without providing information about tests. Lacking any subsidy, the logical step toward supporting such free informational services was to establish not only a collection of samples and references on tests but a sales agency for them. Thus from a very humble start in 1930, with Bregman's Revision of Army Alpha as its only stock in trade, the Tests Division has earned its own way to the position of an important service, publishing, and distributing center for psychological tests and materials, with cost inventories running to several thousand dollars and an annual turnover of hundreds of thousands of tests. Chief credit for the successful development of this Division and the preparation of its increasingly important and useful catalogs belongs to its former head, Miss Wilhelmina Bennett. Upon her retirement, the Division was fortunate in securing Dr. George K. Bennett as its Director and its further growth and usefulness seem assured. Among the accomplishments of this Division may be mentioned the publication of Dr. Gertrude H. Hildreth's "Bibliography of Mental Tests and Rating Scales" and the launching of some twenty useful new tests, revisions and forms by various authors under its own copyrights and of others as principal distributor.

Although the problems of this Division are many, few if any of them are of its own creation. Of the making of tests there is no end and, once they are made and neatly labelled as to what they purport to measure, they go merrily off to market via the route of commercial publishers and royalties to authors. When they are thus available for a few dollars or cents to *anyone*, the belated cry of psychologists that only experienced psychologists should use them does not deter charlatans, or even conscientious amateurs, from having a go at them. In this difficult situation, the Tests Division of the Corporation has consistently sought to serve as a disinterested and impartial advisor on the status and merits of any available tests and their appropriateness for various uses. It need scarcely be said in an article in a psychological journal that this is a thankless, costly, and time-consuming task requiring exceedingly well-informed, patient, and tactful personnel. Inquiries range from the problems of experts in test construction and use to those of persons wishing to purchase some amusing psychological

tests for a party that evening, or of high school superintendents or business executives stating that they wish to install a testing program at once and want suitable tests and all necessary information by return mail. As a corrective for the latter types of requests, the Division calls attention to carefully phrased statements in its catalog regarding the limitations and proper use of tests, recommends the employment of experienced psychologists, seeks to ascertain as well as possible in advance the qualifications of all purchasers and to advise against purchases by unqualified persons. But such measures, of course, cannot prevent the misuse of tests nor their purchase elsewhere by anyone wishing to buy them. Bad as the situation may seem, it probably harms the public no more than patent medicines or amateur doctoring, and there is at least the advantage that, in the competition of a free-for-all market, poor tests and poor testers, although they may flourish temporarily, will sooner sink to their own proper levels.

The making of *good* tests requires more than a stroke of genius when it comes to establishing norms, and herein lies a great opportunity for the Tests Division to promote coöperative work among many psychologists. But here also the funds for such purposes must first be earned from its narrow margin of legitimate profits on such tests as are now in use, and there is the further difficulty of selecting from new offerings not only meritorious work but tests which are most likely to have a market warranting the expenditure of time and money and the enlistment of the services of psychologists in their development. Here too the Tests Division faces unusual competition in the fact that grants are frequently made by foundations to the makers of tests for the work of test construction, etc., and then the tests are subsequently sold at cost by subsidized agencies. The impartiality of the Tests Division's services is well indicated by the fact that it distributes many such tests at a loss as well as many items from other manufacturers on which discounts permit no profit. Hence the remarkable degree of success it has been able to achieve shows both the need for the services it undertakes and the efficiency of its operation. With such funds and limited staff time as it now has, it is endeavoring to improve a number of tests, both by item analysis and accumulation of norms, and has recently obtained the coöperation of some fifty representatives in developing nation wide norms and data on the P.Q. or personality quotient—a test for children devised by Dr. Henry C. Link. Eventually there may come a time when the Corporation will have certain tests in such control that their use by business firms or other organizations

will be permitted only where and so long as psychologists representing or approved by the Corporation are retained. This is at least a seemingly desirable objective toward which to work.

Market and Advertising Research

It is in the field of marketing research that the Corporation has literally led the way for psychologists and made significant contributions both to psychological techniques and the advancement of psychological knowledge. Until recently this field was practically untouched by psychologists. Today it offers many openings and, along with its closely allied lines of social and political research, presents vast possibilities for psychologists. True, many psychologists in the past had undertaken isolated laboratory work on minor problems of advertising, but it was not until Dr. Henry C. Link showed that the *major* problems of selling and advertising demanded consumer studies *in the market place* that the possibilities of 'field work' by psychologists were realized. It was he, too, who led the way in forging tools for such research and rallying psychologists to the attack. Fortunately for the Corporation, and indeed for the advancement of applied psychology in America, his efforts met with gratifying response from many open-minded psychologists and alert students who, with scientific spirit and little hope of financial reward, began in 1932 to take part in experimental studies under his direction. The outgrowth of these studies has been the present eminently successful Marketing Research Division of the Psychological Corporation, which, with apologies to Laird and friendly British colleagues, "puts to shame anything of its kind in England and stands as a virile, living monument to the cooperative work of American psychologists."

If any construe marketing research and the work of this Division as the mere "counting of noses," they are mistaken. The problems in this field go to the very heart of most psychological theory and require the best that psychologists have to offer in the way of techniques and ingenuity for their attack. What makes a million cash registers clatter cheerfully, and what would be the consequences if they ceased? To scorn such problems is to play the ostrich in a society whose betterment depends on scientific effort to solve them. Every purchase is a sample of *buying behavior* of psychological, social, and economic significance. Psychologically it represents the attitudes, opinions and tendencies of consumers, the attributes of the product, the effects of advertising, and many other influences playing upon the purchaser.

Here indeed is a fertile field for psychological investigations and one wherein the cash registers quickly and accurately record results.

The work of the Corporation's Marketing Division illustrates strikingly not only the fine spirit of coöperation among psychologists but the economic value of their joint services and the important contributions to psychological, social, and political knowledge they can make through such coöperation. Well-planned and properly supervised and conducted market studies involving the interviewing of adequate samples of consumers throughout the country are worth considerable sums to manufacturers and advertisers. The Psychological Corporation is now efficiently handling many such studies and stands in the front rank of research agencies in this field of work. Frequently fifty to a hundred psychologists and perhaps a thousand college or graduate students under their supervision take part in some study directed from the Corporation's Main Office. As a means of measuring the trend of buying for many nationally advertised products, the Corporation periodically conducts the interviewing of 4000 different consumers every two months in forty-seven cities and towns properly located according to the distribution of population in the United States. The results are available within one week of the time the interviews are made in the form of a convenient "Barometer Report" which has proven remarkably sensitive and accurate in showing fluctuations in buying trends. This Barometer and other contributions of the Corporation to marketing research have been described in the *Harvard Business Review*, *The Journal of Applied Psychology*, and other periodicals and were presented before the International Congress for Scientific Management held in London in 1935. Among the many prominent companies to avail themselves of the Corporation's services in this field are the American Can Company, the Eastman Kodak Company, the National Broadcasting Company, the Union Carbide & Carbon Company, the Du Pont Company, and the Milk Research Council of New York. For participation in practically all of the marketing research work now done by the Corporation, psychologists and their student interviewers are paid for their services at rates higher than those of most commercial agencies. It must be remembered that the field is a highly competitive one in which the practice is frequently to let various research agencies bid for jobs and in consequence bids for comparable work must be kept in range with those offered by other agencies. Nevertheless, the Corporation paid well over sixty thousand

dollars to psychologists and students participating in its market studies during the years 1935 and 1936.

Social Studies

Early in the course of its experimental studies, the Corporation began including questions on social and political matters in its market studies. The results readily indicated that here was a valuable adjunct to the accurate reporting of public opinion and the study of its fluctuations in relation to various events and factors likely to affect it. Similar studies modeled very much along the lines developed by the Corporation were promptly taken up by *Fortune Magazine*. At the same time that the Corporation was making its studies, Dr. George W. Gallup was also pioneering in this field and his work has now developed into the American Institute of Public Opinion which will probably rank as the outstanding achievement in the social psychology of several decades. Both the Gallup and the Fortune studies have relied heavily on the Corporation's pioneering work, especially its study of scientific sampling, "How Many Interviews Are Necessary for Results of a Certain Accuracy."

Not infrequently consumer surveys lead to special studies of products that can best be conducted in psychological laboratories at universities. Such studies have already been arranged by the Corporation and conducted in coöperation with psychologists at Cornell and Columbia, and they have contributed results not only of commercial value to the companies concerned but of significance for the improvement of psychological methods. Of the Coca-Cola taste study, for example, Dr. Dallenbach stated: "It represents one of the most important contributions in the field of psychophysical techniques." Although the Market Research Division of the Corporation may seem at times to be the tail wagging the dog, it is greatly to 'his' benefit and most nearly approaches the vision of the founders for coöperative work by many psychologists and the use of university laboratories for research on practical problems contributing also to the advancement of psychological knowledge.

The Chicago Office

In Chicago, Dr. Robert N. McMurry is serving effectively as the Executive Secretary of an office of the Corporation established there in 1935,[4] with the aid of funds advanced by the New York office. This venture was launched with the fine coöperation and rendering

[4] Address, 310 S. Michigan Blvd., Chicago, Ill.

of part-time services as staff by the members of a committee of psychologists in Chicago who have accepted a large share of the Corporation's responsibility for this office: Drs. S. N. Stevens, Chairman, A. W. Kornhauser, Grace B. Manson, and Andrew W. Brown. Industrial or marketing research services have already been rendered for a number of business organizations by this office and its work for the Hoover Company on a selection and training program for salesmen has been outstanding. It has not as yet set up a separate Division, or Psychological Service Center, for school and personal services, but it may do so as funds and personnel for the purpose become available. Despite its early financial difficulties, the prospects are now bright that this Chicago office will not only be able to pay off its obligations to the New York office but will become a source of valuable influence as well as income for the Corporation as a whole.

The San Francisco Office

There are similar prospects for an office of the Corporation in San Francisco which, without any financial aid from headquarters, was opened there in 1935 by Dr. Edward G. Stoy, after previous conferences and some experience with the staff in New York, and with the approval of the Corporation's Executive Committee. The services of this office are now retained by several business concerns and Dr. Stoy is also in charge of the guidance and placement services of the National Youth Administration in California. For marketing research work, some of which has already been undertaken locally, Dr. Stoy has close at hand at the University of California in Berkeley the able assistance of Dr. C. W. Brown and his corps of interviewers with experience in the Corporation's nation-wide surveys. Personal counseling services are also rendered at the office although no separate division for them has as yet been set up. It appears likely that under Dr. McMurry's and Dr. Stoy's direction respectively, staffs can be built up at both the Chicago and San Francisco offices along the lines adopted in New York, the chief difficulty being to find capable staff associates able to earn their own way on a commission basis.

With regard to business arrangements, both of the above offices are under agreement that no financial obligations beyond their own resources shall be assumed in the name of the Corporation unless approved in advance by properly responsible officers of the Corporation at its Main Office. Similarly the Main Office is to be advised regarding the work to be undertaken by these offices and the arrangements made with the organizations for which it is to be done. In compliance with corporation tax laws all offices operating under the name of the Corporation must submit annually to its headquarters all financial and other data necessary to corporate control and preparation of the Corporation's income tax return. The present arrangement regarding the net earnings of these offices is that they shall be allowed to accrue for the expenses and development of these offices but shall always be subject to action of the Corporation's Board of Directors in allocating reasonable portions of them for other corporate purposes. If, after further trial and experience, these arrangements prove unsatisfactory from legal or other standpoints they may be revised, or possibly offices of the Corporation may become independent corporations in different states with control by the parent organization limited to its holdings of voting stock in these separate corporations.

Other Branches

In addition to and to be distinguished from the above mentioned offices of the Corporation, there are at present *Psychological Service Centers* in St. Louis, Minneapolis, and Milwaukee, and a *Psychological Institute* in Washington, D. C., which, under the Corporation's former policy were designated as "Affiliated with," or "Under the Auspices of, The

Psychological Corporation of New York." These Centers are operated independently by Dr. H. Meltzer, Dr. A. R. Root, and Dr. S. M. Shellow respectively, and the Institute in Washington, now engaging only in research in the field of education and the distribution of the tests and materials incident thereto, by Dr. L. J. O'Rourke, who is himself a member of the Corporation's Board of Directors. The work of these Centers and Institute has been highly creditable to the Corporation. Particular mention should be made of Dr. O'Rourke's outstanding and basic research work in the field of English usage. Since 1930 the Corporation has endeavored, within its resources, to facilitate this work and in 1930 it conducted in close coöperation with Dr. O'Rourke a nation-wide study of English usage on a self-financing basis in which tests prepared by Dr. O'Rourke were taken by over a million children. In view of the significance of this work, the Carnegie Foundation has subsequently made a series of grants for its continuance and proper fruition. These funds have been held by the Corporation at minimal administrative expense and subject entirely to Dr. O'Rourke's use in this work for which the Institute in Washington serves as headquarters. Thus the work of this Institute and its affiliation with the Corporation may be regarded and treated by the Corporation as a special case. The present status of the other Centers, however, is inconsistent with the policies outlined above and now being adopted by the Corporation. Under these policies the term "Psychological Service Center" as related to the Corporation signifies primarily clinical, school, and personal counseling services, and no such Centers are to be sponsored by the Corporation except where they are in immediate association with, and under the direct control of, one of the Corporation's offices as is the case in New York. Consequently the possibility of changing the status of the above mentioned Centers to that of offices of the Corporation under the same arrangements as those applying to the offices in Chicago and San Francisco is now under consideration or active negotiation.

The preceding discussion portrays very briefly the Corporation's past and possible future course of development in the fields of clinical, 'personal service,' and industrial psychology. These are fields wherein psychologists are apt to work most effectively as individuals. But there remain for consideration fields wherein psychologists may effectively work together in researches and services affording opportunities for coöperative joint efforts. These opportunities appear most clearly in the operations of the Corporation's Tests and Marketing Research Divisions.

In summary it may be said that, despite discouraging early experiences and many currently difficult problems, the Psychological Corporation has not only been able to survive without the aid of subsidies but, by being of practical usefulness to business and industry and individuals, to provide remunerative work for many psychologists, to influence teaching in many classrooms, and to open new fields of opportunity for psychologists. It will continue to succeed and to contribute to the advancement of psychology so long as many psychologists maintain interest in its aims and are willing to work coöperatively in the spirit of science for their achievement.

ON 'UNBEARABLE' IDEAS AND IMPULSES

By John Rickman, London
Editor, *The British Journal of Medical Psychology*
(Founded in 1920)

If at some future date, a student wished to survey the development of psychology in the last fifty years he would find much to guide him from a study of this Journal, which is remarkable not only for its scope but for the fact that it has given an early welcome to so many ideas which have exercised a lasting influence on our science.

Before about 1900, psychology was in the main concerned with the phenomena of consciousness, with introspection and cognition and with such experiments as would throw more light on these problems. The question of pain, when considered at all, was treated as a mode of sensation, it did not include 'mental suffering.' But suffering is to the ordinary man most closely associated with mental phenomena, physical pain is a rare and unwelcome visitor and the victim of it hastens for help or takes care to avoid the occasion of it. In the case of mental suffering the victim does not call in aid till he must, indeed he often seems compelled to take the very path to meet it and even at times to welcome it. It would be hardly an exaggeration to say that to the ordinary man a study of the mind that did not in any great measure deal with the *sufferings* of the mind would be regarded as dealing with an abstraction. This view is not held as a reproach to the psychologist by the ordinary man, for experience shows that the moment that mental pain is the subject of study he takes up a contrary attitude, saying that the topic is being mishandled and emotion is given too great prominence to the belittlement of reason. Another factor which contributes to the difficulties of studying mental pain is indeed a strange one. Though willing enough to speak in general terms of misery as an inevitable part of our human lot or to call an individual 'shallow' or 'inhuman' who has no understanding of mental suffering, yet the moment that this suffering is an object of investigation its victims are regarded as abnormal, for 'psycho-pathology' is thought of as a science of mental disease rather than the study of mental suffering and its deductions are regarded as in no way valid for the normal mind or the

normal man, who of a sudden is pictured as guided by reason and predestined to a life of pleasure. Even psychologists have shared these contradictory views.

The rescue of psychology from the predicament is due almost entirely to the work of Freud, who gave an account of his way out of the difficulty in this JOURNAL twenty-eight years ago in "The Origin and Development of Psycho-Analysis,"[1] which was, one is specially glad on this occasion to recall, the first general exposition of psycho-analysis ever published.

Psychologists as a rule not only choose their field of study but can within limits regard the person whose mind they are going to investigate as 'laboratory material;' the research worker is in more or less complete control. In the case of clinical psychology it is otherwise, the person whose mind is to be investigated is usually in a nervous state and does not know what is the matter with him. He resents or is suspicious of 'interference' and yet is driven by mental pain to discuss with his physician the most distressing ideas from which he suffers, much though he would prefer that his mind should be 'let alone.' At first sight the situation seems unpropitious for discovery, and indeed if the psychologist behaves in a clinical situation as if it were a laboratory that ends the chances of his keeping the 'laboratory material' within view at all—the patient goes to someone who will 'understand him.' That is to say the physician interested in research and the patient interested in relief from suffering appear to pull in opposite directions; the divergence of aim can however be resolved. If the physician keeps one point clear in his technique, things cannot go hopelessly astray, viz. for when a patient is told to say anything that comes into his mind, keeping nothing back on grounds of its apparent irrelevance or on any other pretext, there *is* a connection between his associations even though it may not be immediately obvious. The patient on his part will have but little interest in research, and it is invariably found (if the physician does not interfere with the free flow of the patient's ideas) that the patient will keep the personal side of the relationship to the fore, showing either warm affection or bitter hostility or a mixture of both to the person who is treating him.

Experience shows that if the physician can give an equal attention to every manifestation of the patient (friendly or hostile, 'interesting' or 'irrelevant') he will discover that the patient is struggling with 'unbearable' ideas and impulses, and is also struggling with a double

[1] Sigmund Freud, *op. cit.*, this JOURNAL, 21, 1910, 181–218.

tendency, to bring these ideas and impulses into the open at the same time being full of apprehension about doing so. The physician for his part, perhaps not unnaturally, often finds within himself (if he has any capacity for insight) a great aversion to dealing with unbearable ideas and impulses. If he glosses over these or belittles their importance the therapeutic effectiveness of his work falls, and he gets no results in his cures or in his research.

The important field of psychology that deals with unbearable ideas and impulses was no man's choice, therapeutic necessity forced it on the physicians, and, one instantly assumes, that these ideas and impulses are thrust on the patients by illness. A great deal that is important for the future of psychology hangs on that assumption—it may be wrong.

Research in psycho-pathology does not in essentials differ from other kinds of research; the important thing in every case being the removal as far as possible of the influence of factors which cannot be observed or controlled. The research situation in the case we are considering, which is also a therapeutic one, consists in the patient being given the most favourable opportunity under observation of giving free play to his associations; this means that the physician must keep himself and his own ideas in the background and confine himself to pointing out connections between the patient's ideas which the patient himself fails to observe.

It is found that a situation then arises in which the patient finds himself in an 'unbearable' position. Ideas and impulses come to the mind which are 'intolerable,' and a great deal of manoeuvring takes place in order to reduce the mental strain. It is a close observation of this manoeuvring which has made psycho-pathology such a fruitful field of discovery. If the physician's attitude of benevolent detachment is not changed by the behaviour of the patient, *i.e.* if he can retain his objectivity, a new situation arises. The patient is found to be responding to an *imagined* environment as well as to that which exists objectively, and the imagined environment is conditioned by ideas and impulses which as later work would show belong to the period of the patient's infancy. It is at this point that the investigation usually gets out of hand, the physician often begins to tell his patient that he is not distinguishing between reality and phantasy and that in fact he himself is not as his patient imagines him to be. This may be true objectively but the thing of importance is that the patient has such an idea at the moment about his physician, and that is what has to be

explained. No amount of hedging about the 'irrelevance' of a mere transient thought will do in psycho-pathology any more than the mere transience of a flash can be brushed aside when investigating an electrical circuit.

Only if the physician can tolerate the intrusion of these intolerable ideas and impulses into the intricate personal relationship which the patient's attitude creates will he be rewarded with an insight both into the origin and the part they play in the patient's symptoms and mental development.

Clinical View of 'Unbearable Ideas'

If we carefully use the special instrument of clinical research mentioned above for investigating the deep levels of the mind, we find a type of mental function quite different from the conscious adult one,[2] and our present subject—unbearable ideas—takes on a new aspect.

An idea or an impulse is unbearable when it inevitably involves total loss of security. This definition applies both to conscious and unconscious modes of mental function, and it is valid for actual, *i.e.* objective, and for imagined or subjective sources of insecurity or danger. Freud distinguished between fear,[3] where the source of danger is known to the conscious mind, and anxiety where it was not known; in the former case the appropriate response was flight or some manipulation of the external situation (Rivers' 'manipulative activities'), in the latter case no activity directed to the external world removes the source of mental tension.

The feeling of *mental insecurity* belongs to the latter category of events. If the danger to life (or to what is regarded as precious) is external, the mind is not benumbed—unless for reasons to be given later it is inhibited by the unconscious—and it deals actively with the trouble. The sources of danger which give rise to the 'unbearable ideas' lie in the unconscious. These may be summarized under two headings, loss of satisfaction from the object (due to a change in the object as it is conceived in the unconscious) and loss of the capacity to love (due to a change in the self); either of these will bring about an unbearable situation and the mere thought of it is felt to be intolerable.

We must distinguish carefully between conscious and unconscious modes of object relationship when using this simple formulation; it is

[2] The most concise yet full statement of this is given in A General Selection from the works of Sigmund Freud, *Psycho-Analytical Epitomes* 1, 1937.

[3] *Op. cit.*, last section.

not a case of loss of satisfaction from *any* object, but from that object which lies in the focus of the unconscious desire. Our orientation to the problem must be guided by the subjects' unconscious phantasy.

If we make use of the researches into the early mental development of the child, for which we are principally indebted to Melaine Klein,[4] we shall have a clearer understanding of the character of loss of relation to the object which is so intolerable.

The infant, whose impulses swing with uncontrolled strength from love to hate and hate to love, is at first oriented to one object—the breast—and this object is loved with a consuming love or is made the point of unbridled attack in its phantasy. In a gust of hate it destroys in its imagination the very object on which its life and its satisfaction in life depends. Its ambivolence and the fact that is is a mouth-governed creature cause its strong impulses to be a source of danger to itself—not in an objective sense but in its phantasy, for imagination begins early. In other words the experience of the small infant leads it to regard its own impulses as dangerous to its security, for in its phantasy-experience, which to it is a part of reality, love and hate both tend to the disappearance of the object of desire.

We are not here concerned with the steps by which the sense of security is built up, but only with the continuing effect in the unconscious of the early dangers to a sense of security. Man's behavior may be described as the resultant of his response on the one hand to the current, objective, conscious situation in which he finds himself and on the other hand to a series of past situations, which are for the most part unconscious but which colour his subjective attitude to his environment; further his response to the present and past situations is determined by two factors, the (external) influence of the environment and his (inner) instinctual forces. There is thus in the adult a double object, the actual objective or conscious one and the image of a past object reactivated in the unconscious; and a double orientation, towards the (external) object, only existent in the present, and towards the (internal) image persisting in the unconscious from the past. It is necessary to consider both if one is to understand the nature of the unbearable, because the threat to the object which causes the intolerable mental pain is in all cases the phantasy of attack upon the image of a 'good' or helpful, *i.e.* satisfying, object of early experience. The unbearable sense of sin from which the melancholic suffers with such

[4] Melaine Klein, *Psycho-Analysis of Children*, 1932, 1–393; see also M. Klein and J. Riviere, Love, hate, and reparation, *Psycho-Analytical Epitomes*, 2, 1937.

severity is to be explained, not as a remorse for past *deeds*, but as the self-punishment under a load of guilt for *phantasied attacks* upon the person who in the past was most loved, most needed and most hated. In the same way we can trace the sense of utter desolation which afflicts the paraphrenic, that universal chaos and destruction of all good and living things, as the experience (in phantasy revived during the crisis of illness) of those early sadistic phantasies in which the living were bitten up, torn to bits, and annihilated.

The major mental illnesses provide us with the most conspicuous examples of unbearable ideas, but if we carry the analysis of mind to its deeper, *i.e.* unconscious, levels, we find even in the normal person evidence that he also passed through a period in which he strove with mental pain that proved well nigh unsupportable.

The consequences of these discoveries are likely to be momentous for psychology. In the first place man's colossal appetite for destructiveness is shown in a new light, for though it undoubtedly springs from a destructive impulse, it belongs in the main to a period of development when the instinctual impulses are not integrated. Secondly, man's enormous capacity for guilt is found to be part of a reaction against the destructive tendency, and, thirdly, man's unique capacity for constructiveness is found to be stimulated by a wish (guided by the impulse of love) to restore and reëstablish what had been in phantasy attacked and smashed in the phase of unintegrated instinctual expression.

This conception of the part played by unbearable ideas in the development of 'normal' impulses in normal people is of assistance to us in understanding the 'drive' in artists to produce 'eternal' or 'living' works of art. It seems at least likely that the creative impulse is spurred on to even higher achievements by the need to reëstablish the sense of 'indestructable life' partly at any rate because of the experience in the (unconscious) phantasy of the unbearable pain caused by the destructive phase.

Postscript

It is gratifying to recall at the time of a Jubilee that within the first decade of the period in question psychology extended its boundary to include territories of the mind cut off from consciousness because the ideas and impulses in that region were unbearable, and now in the last decade of the period we are beginning to understand not only more about mental pain but also to realise that the mind can recover from its travail and use its bitter experience in the construction of its highest and most precious achievements.

PRIMATE COÖPERATION AND INTELLIGENCE

By Robert M. Yerkes, Yale Laboratories of Primate Biology
Editor, *The Journal of Comparative Psychology*
(Founded in 1911)

I

This chapter from the experience of a student of animal behavior is offered as prolegomenon to the experimental analysis and description of those forms of social behavior which constitute or condition co-operation. It is written in grateful memory of the professionally immortal founder of the journal whose semi-centennial this volume celebrates. G. Stanley Hall wrought more wisely than he could know when he established, and through lean and vexatious years supported, *The American Journal of Psychology*. It pleases me to believe that as one who prized genetic description he would have been keenly interested in this contribution of fact and hypothesis and generously appreciative of its values. I have had in mind, as I wrote,[1] his famous Seminar in Psychology.

For one or another scientific purpose, I have used in the last forty years types of organism ranging from invertebrates such as jelly-fish, crustacean, and worm, through the several orders of the vertebrates, to the primates. Experience has taught me two incomparably profitable lessons. To learn truly about the behavior of an animal one must be able to observe accurately, inclusively, relatedly, and to understand it one must be capable of establishing a sympathetic rapport which assures mutual naturalness of attitude and action. The student of animal behavior who is unsympathetic with his subject, like the artist who lacks feeling for his, is cut off from invaluable aids to insight and creative effort. The psychobiologist as naturalist should be also artist, clinician, and experimentalist.

Because for a great many years I have worked almost exclusively with primates, and more especially because their behavior well exemplifies or illustrates the facts, principles, and pragmatic lessons

[1] The substance of a paper presented to the American Psychological Association, Hanover, New Hampshire, September 2, 1936.

which I wish to set forth, I have drawn my materials for this article from my own and my associates' experience with chimpanzees in the Yale Laboratories of Primate Biology. There, thanks to the courage and faith of the administration of the Rockefeller Foundation, we have been enabled to labor continuously and intensively with a colony of these behaviorally complex and scientifically valuable anthropoid apes.

II

A brief account of the evolution of methods of handling chimpanzees in these laboratories and what we have learned during the process will serve at the outset to establish the occurrence of coöperative behavior in these animals and to indicate the intimacy of its relations to intelligence.

When in 1925 we undertook to keep and use a group of young chimpanzees for biological inquiry, we assumed from what was then known that restraint and compulsion would be essential to economy of effort and directness of achievement. We therefore designed reaction and observation chambers, experiment cages, restraint chairs, standard observation grilles, hoods, and leashes. These devices proved of unexpected value as exhibiting to us the psychobiological traits of our animals.

In connection with anthropometric observations, we early discovered that any form of compulsion tended markedly to increase the variability of our measurements and correspondingly to lessen their comparability and reliability. Harold C. Bingham finally succeeded in training four subjects to submit willingly to instrumental measurements,[2] and in general to coöperate effectively with him. But this success was achieved at a cost of time and patience which would have been prohibitive had we not been able to use the subjects intensively and for a relatively long time. It was at this stage of our working acquaintance with the juvenile chimpanzee that we discovered the special applicability in our situation of the adage, "The more haste, the less speed," and became convinced that patience is indeed a consummate virtue in one's dealings with this ape.

As our experience accumulated and became more varied, we discovered also that adaptation should be mutual instead of limited to the subject alone, and, further, that it should occur progressively in experimenter and subject. Gradually our faith in our initial assumption

[2] H. C. Bingham, Observations on growth and development of chimpanzees, *Amer. J. Phys. Anthrop.*, 13, 1929, 436 ff.

about the necessity for restraint and coercion weakened, and the following events completely destroyed it. For a study of auditory acuity, James H. Elder,[3] in the then current tradition of the laboratories, attempted to place his subjects in a specially constructed restraint chair, on the design of which Henry W. Nissen and the writer had spent considerable time. The animals objected strenuously, and their adaptation to this apparatus was so slow and uncertain that the experimenter finally decided to try treating them as he would human subjects, by seating them in a chair before the response key, placing the stimulus-delivering earphones on the head, and proceeding with the routine of experimentation. Success was almost immediate, and the restraint chair was never again used in the investigation. This experience naturally gave Dr. Elder confidence in the ability of the chimpanzee to understand the requirements of the experiment and also in its readiness to coöperate with him under reasonably favorable conditions.

Subsequently, encouraged by Dr. Elder's remarkable success, other members of the laboratories discarded in varying degrees the method of restraint and relied increasingly on obedience to commands and coöperation. Notable in this series of demonstrations of coöperativeness is the work of Milton C. Forster on the measurement of time of reaction in the ape,[4] and of S. D. Shirley Spragg on maze performance as a method of exhibiting anticipatory response.[5]

In this stage of our methodological progress, all was not as simple and straightforward as the foregoing paragraph would seem to imply. Failures and disappointments were at least as frequent as successes. Viewed in retrospect, this was due to the inadequacies of our knowledge of our subjects and of our practical insights. Particularly baffling and puzzling at that time was our inability to induce young subjects who were not accustomed to living alone to work quietly and efficiently in isolation, or in some cases even to tolerate being left alone. Some of our animals, in the age-range one to four years, would cry or scream loudly for long periods and day after day, while refusing to attend to the experimental situation. We then characterized this behavior as an expression of stubbornness or wilfulness, whereas today we confidently

[3] J. H. Elder, Auditory acuity of the chimpanzee, *J. Comp. Psychol.*, 17, 1934, 157–183; The upper limit of hearing in chimpanzee, *Amer. J. Physiol.*, 112, 1935, 109–115.

[4] M. C. Forster, Temporal relations of behavior in chimpanzee and man as measured by reaction time: I. Simple reaction time, *J. Comp. Psychol.*, 20, 1935, 362 ff.

[5] S. D. S. Spragg, Anticipatory responses in serial learning by chimpanzee, *Comp. Psychol. Monog.*, 13, 1936, (no. 2), 1–72.

diagnose many such cases as lonesomeness or nostalgia—a miserable state in which the subject cannot work normally and should not be expected to.

Experience year by year modified our point of view and expectations, and, correspondingly, our practices. Within a decade our original assumption of dependence on compulsion had been supplanted by one which initially would have seemed preposterous to us. Stated briefly and dogmatically, our present rule of action reads: expect and command voluntary and intelligent coöperation. Basic to this new assumption is the knowledge, achieved by much labor, that the chimpanzee is capable of sufficient understanding and insight in varied behavioral experiments to enable it to coöperate serviceably with the investigator, if it so desire, and, further, that it is the privilege and opportunity of the experimenter to command its willing and eager adaptive coöperation. Whereas initially varied forms of restraint and the imposition of the experimenter's will by force were dominant, today our working situations are characterized by a large measure of freedom of action and intelligent coöperation on the part of our subjects.

For a multitude of practical and scientific needs, our adult subjects, who are too strong to be handled safely, have been trained to desired forms of behavior. The following are examples. Several years ago, O. L. Tinklepaugh trained several of the mature females of the colony to present voluntarily for genital examination and to permit sampling of the vaginal content.[6] Somewhat later James H. Elder accustomed several members of the group to rectal injection in order that anesthesia might be effected without the use of force or of an anesthetization chamber.[7] This achievement was especially important as offering convenient means of separating infant from mother during the period in which the former almost continuously clings to the latter and is carried about by her. More recently, S. D. Shirley Spragg has demonstrated with a few mature or nearly mature subjects the practicability of training them to entirely voluntary coöperation in the procedure of hypodermic injection and in skin puncture for the collecting of blood samples.[8]

These three types of procedure—genital examination, anesthetization

[6] O. L. Tinklepaugh and E. Van Campenhout, The vaginal cell content of the mature and immature chimpanzee, *Anat. Rec.*, 48, 1931, 309–322. Also unpublished observations.

[7] J. H. Elder, Methods of anesthetizing chimpanzees, *J. Pharmacol. Exper. Therap.*, 60, 1937, 347-357.

[8] In connection with an unpublished study of the nature of morphine addiction in chimpanzee.

by rectal injection, and hypodermic injection—would seem to represent steps in difficulty or exactingness of requirement. In each case our procedure has been the same: to suggest by word, posture, pantomime, or actual performance what is required of the subject and to help it to associate compliance with reward. Ordinarily bits of food or words of commendation serve adequately as motivating rewards. In a word, our subjects are shown what they are expected to do, or what is required of them in an experimental situation, and enabled to discover by trial that there is nothing to be feared and that the final outcome is likely to be agreeable.

Similarly, utilizing the suggestibility, imitativeness, intelligence, and desire of our subjects to obtain rewards, we have trained the members of the colony to enter a transfer crate on opportunity and at command, so that they may be weighed expeditiously and without risk of damage to scale or observer; to enter special experiment boxes; to submit to collar and leash; and to coöperate in many like ways to our very great advantage.[9]

It has been discovered also that the chimpanzee which is accustomed to life in captivity and to human caretakers, if in need of aid because of injury or illness, ordinarily will quietly permit examination or even painful manipulation and treatment, and that it may at times exhibit indications of appreciation of the service rendered it. Before urgent need, shyness and timidity may disappear and a subject act as if perfectly at home even with a complete stranger, who in the presence of persons with whom it is familiar offers medical or other aid. At times we have been amazed by the willingness of our subjects to submit to unusual and painful treatment during illness or following injury. By contrast with such behavior, we must emphasize the fact that except when need is evident to the individual, coöperation in medication, or in any manipulative treatment, is likely to be entirely in abeyance and extremely difficult to secure.

Such experiences as are exemplified by the instances cited have convinced us, in the course of years, of the possibility of indefinitely extending the scientific usefulness of chimpanzee simply by learning how to command its intelligent and voluntary coöperation. The instances offered as illustrative of the behavioral principles in point have been selected from many and varied experiences. Only a small part of our

[9] As a matter of course, there are refractory individuals and we occasionally fail utterly to command coöperation. Such failures often are much more puzzling than are our successes.

observations have been published. Largely as a result of our cumulative discoveries and insights relative to chimpanzee nature, we are able to do with them readily today what a few years ago we certainly should have branded as impossible. Then we felt confident in our relative ignorance; today we are unwilling to set limits to possible progress because we have seen too many transcended. Our experience amply justifies the remark that insight, ingenuity, and patience are incomparably more important in connection with the use of chimpanzee as material of psychobiological research than are impulsiveness, abundance of energy, and driving ambition for achievement.

III

Our reasonably extensive experience in keeping and using them in research points very definitely to *security*, *confidence*, and *understanding* in chimpanzees as essential psychobiological conditions of their cooperation with man. Each merits description, exemplification, and discussion.

Security designates the experience-behavior pattern of personal safety and at-homeness. Although timidity is natural in the infant chimpanzee, caution and specific fears (avoidances) appear in general to be parent-taught. At birth the infant, strictly speaking, trusts either nothing or everything. After a time it comes to rely on the mother, and gradually its sense of security comes to include other sections of the environment. It is not a matter of first relying confidently on everything and of gradually learning to distinguish the harmful from the beneficial. Instead, trusts and fears are acquired day by day through social tradition as represented in the parents and through other forms of individual experience. The more discriminatingly and wholeheartedly an individual comes finally to trust its environment, and especially the social segments thereof, the more freely and efficiently it acts.

During the first few weeks of life dependence upon the mother is complete.[10] The infant clings to her tenaciously whenever she is in motion, and often also when she is quiescent. Even after locomotor independence and ability to run about and climb freely have been achieved, the youngster rushes to the mother whenever alarmed. For several months the parent continues to be a haven of safety and security. The query is pertinent: Is it possible, and at the same time practicable,

[10] R. M. Yerkes and M. I. Tomilin, Mother-infant relations in chimpanzee, *J. Comp. Psychol.*, 20, 1935, 321–359.

to shift the dependence of the young chimpanzee from parent to human caretaker or experimenter? The answer from our experience is clearly affirmative. Indeed, that is precisely what the investigator must accomplish if he would work comfortably, efficiently, and reliably with infant or juvenile apes.[11]

Unless accustomed to being kept alone, it is relatively difficult to use an isolated chimpanzee of less than three years of age in experimental situations; and it may be highly dangerous to approach a mature animal because of its caution and fears. The one complains as does the timid human infant under like circumstances, and the other defends itself. Either form of behavior is unfavorable to experimentation. By many costly failures we have learned that our first task is to make our subjects feel entirely at ease in any observational situation, and wholly safe in the presence of the experimenter. The replacement of timidity by security is a long step toward the achievement of coöperation.

For a surprisingly long time, we mistook caution and fear for contrariness, obstinacy, or stupidity. The persistent whining, screaming, and struggling of a juvenile subject when we attempted to work with it seemed like perversity, and so likewise the violent hostility of the adult. It required long experience to convince us that the difficulty was in us, not in our subjects.

To the inevitable question, how develop the state of safety or security in an individual, our generalized reply is by giving it abundant opportunity to discover for itself that there is nothing harmful or disagreeable about the situation in which it finds itself, and that the experimenter desires to be friendly, kind, and helpful. Once the animal has satisfied itself that there is nothing to fear or mistrust, security and at-homeness begin to assert themselves and shortly it is likely to seek, or even beg for, what it previously avoided. It is precisely as in the case of the human infant or young child. If anything or anyone strange and unfamiliar is abruptly thrust upon its attention, timidity or fear may assert themselves, whereas if the presentation be gradual and the individual has ample opportunity to observe, inspect, and make tentative exploratory advances, expressions of familiarity and security become dominant and the individual obviously enjoys what otherwise would have disturbed or distressed it.

To attempt to distinguish confidence, our second condition of cooperation, from security may at first seem academic, for obviously the

[11] C. F. Jacobsen, M. M. Jacobsen and J. G. Yoshioka, Development of an infant chimpanzee during her first year, *Comp. Psychol. Monog.*, 9, 1932, (no. 1), 1–94.

two are mutually dependent patterns of experience-behavior. Indeed, it is only as confidence in self and not-self is acquired that at-homeness and security come into control and the animal to behave naturally and without constraint or apprehension. However, to judge by our experience, it is not sufficient to free the chimpanzee from general timidity or from specific fears. This is merely a preparation for the development of perfect confidence, and only when the latter has been gained does the animal come to act with abandon, freely, naturally, playfully if young, boldly and without restraint if mature or old. One may think of timidity and trust as extremes of a reaction series. By eliminating his subject's fear of him, the observer achieves the zero point of disturbance; whereas by establishing perfect confidence, he measurably increases the value of a subject in varied observational situations. It is, then, our first task to eliminate or minimize general timidity and specific fears, and our second to win the confidence of our subjects in such measure that they can act without disturbing inhibitions.

Although the chimpanzee does not speak, it does communicate. As unmistakably as do we it may inquire of man or fellow ape: May I trust you? Are you friendly in intent? Common gestures of cautious inquiry are the extended hand on bent wrist and the offer of hand to mouth. In either case the questioner places itself momentarily at the mercy of the animal approached, for the open palm is toward the questioner. From childhood to senility these gestural inquiries are exhibited. We have observed them hundreds of times. In an extreme instance, an adult ape may place its hand in the mouth of a stranger, or similarly offer it to the lips of a person who is acting aggressively. There are, to be sure, times when gestures such as these are manifestly intended as apologies or as solicitation of friendly favors. Ordinarily confidence must be deserved, as well as solicited or humbly sought, and in no case may it safely be demanded.

There is observational basis for the statement that friendliness, trust, and confidence are proportional to the animal's experience of need. Therefore if one wishes promptly and with minimal effort to win the devotion, dependence, complete trust, and lasting appreciation or affection of a chimpanzee, he can have no better luck than opportunity to rescue it from some dangerous or painful predicament or to render obviously important medical, surgical, or nursing service. Even to happen upon and to be able to lend support to an individual which is greatly stirred emotionally by fear or anger may win its appreciative allegiance. I have had a sick and miserable little chimpanzee pat me on

the shoulder gently in return for the kindness of considerate handling. In our daily contact with these animals we have come to rely, in the measure of our individual wisdom, upon fairness, justice, patience, friendliness, and sympathetic interest so far to command their confidence that they will work with and for us effectively.

Difficult though it may be to believe, strong antagonisms toward man appear to be less frequent in chimpanzees than are correspondingly strong attachments, and neither disciplinary nor coercive measures necessarily lessen friendliness. As evidence it may be reported that vindictiveness and retaliatory behavior have rarely been noted in the scores of instances in which a stream of water, burning torch, a live snake or tortoise, or pistol fire have been used to induce a refractory animal to conform to the will of its caretaker or observer. There are individuals of extraordinarily strong will among apes as among men, and negativism is occasionally observed. Mischievousness also is commonly exhibited, and it is perfectly natural that the inexperienced person should at times mistake it for wilfulness or negativeness. Maliciousness and viciousness are almost unknown to us in our captive specimens.

Confidence in a person, backed by a sense of security, does not necessarily imply willingness on the part of a chimpanzee to subordinate its desires to those of the experimenter when they happen to conflict. Some individuals may do so readily and promptly, whereas others at times cannot be persuaded or even forced to do so. Mutual trust is the indispensable basis of friendliness; yet even without it working relations may be attained if the worker is cautious, experienced, and patient. The risks of misadventure, and of serious mishap, under such circumstances, are much greater than they should be, and it also is relatively unpleasant and uncomfortable for the investigator, since neither he nor the subject can tell what to expect of the other.

As to understanding, the third and last of our suggested basic conditions of coöperation, our experience is particularly illuminating and valuable practically, for it appears that this form of intelligence may condition security and confidence as well as coöperative activity. We lack measurements, but our varied observations suggest that the rate and limits of development of security, confidence, and coöperativeness vary directly with the intelligence of the subject. Although it is nowadays the fashion to belittle the hypothesis that levels of intelligence correspond to diverse neural mechanisms and to assume that organic types differ only quantitatively in adaptiveness, alertness, and under-

standing, the writer, on the basis of his observational experience, supports the opposite contention.[12] It is his present conviction that frog, rat, dog, monkey, ape, man, represent as many varieties or distinctive types of intelligence and degrees of, or approach to, ability to understand experimental situations. Anyone who has worked intensively with representatives of these types of animal surely will hesitate to assert that a given problem can be solved by any one of them if sufficient time and opportunity are provided. With this preamble, I submit that chimpanzee possesses kinds and degrees of behavioral adaptiveness which are known in no other type of organism than gorilla, orang-outan, and man.

Whereas in the experimental handling and use of most sorts of laboratory animal no effort is made to render the working situation intelligible and meaningful to the subject, and thus to facilitate its general adaptation, in the case of our chimpanzees we have demonstrated times without number that it is eminently worth while to try to show the subject what is expected of it, and, in general, to aid it to grasp the intent of what is demanded by the experimenter, instead of merely waiting for it to discover everything for itself unaided. There are, to be sure, investigations in which this practice is illegitimate, but we have found in general, as did Köhler,[13] that it may very greatly facilitate progress in experimentation.

It is our present experience-proved conviction that after a chimpanzee has been perfectly tamed, rendered at home in its laboratory environment and with the investigator, it is wholly desirable to substitute demonstration and patient training by example for the usual let-it-alone, cut-and-dried, standardized procedures which we use with such laboratory animals as mouse, rat, rabbit, or guinea pig. This procedure actually is our nearest equivalent to the verbal description and instructions which we ordinarily offer the human subject, and in our use it is strictly comparable with the treatment which we should expect to accord a human infant or defective incapable of verbal communication.

The understanding of the chimpanzee is so far different from our own in kind and degree of applicability that often where we least expect it our efforts to show or teach an individual fail utterly, and

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[12] R. M. Yerkes, Modes of behavioral adaptation in chimpanzee to multiple-choice problems, *Comp. Psychol. Monog.*, 10, 1934, (no. 1), 96 ff.

[13] Wolfgang Köhler and J. F. Szymansky, Die Methoden der psychologischen Forschung an Affen, *Abderhalden's Hdbh. d. biol. Arbeitsmeth.*, Abt. VI, D, 1, 1921, 69–110.

just as frequently we are pleasantly surprised by facile successes. Commonly there is marked improvement in ability to profit by tuition as the experience of an animal in experimental situations is extended. Otherwise expressed, general adaptation is progressive. Indeed, it appears that an individual may for years continue to increase in usefulness as subject of experiments. This is a possibility which has not to our knowledge been established for any other laboratory animal.

It is my obligation to mention in this connection an almost universal experimenter's error, and to formulate a rule which observational work with animals has imposed upon me. The error is that of relative ignorance of the nature and status of the subject of observation and resultant lack of essential basis for methodological adaptations and sympathetic insights. The rule, which is significantly related to this shortcoming in the experimenter, may be stated thus: In seeking knowledge and understanding of an animal's behavior, study much rather than many, for more profit may be gained from one rat or chimpanzee intensively studied than from a thousand "run" through maze or mill semi-mechanically and for statistical ends.[14]

Undeniable there are situations in which the investigator, without unreasonable risk of penalty, may be largely ignorant of the traits of the type of organism which he is using, and even of the characteristics of the individual. But, in general, ignorance is dangerous, and lack of understanding decidedly unfavorable to the discovery of the truth and the achievement of new insights. For every case there is an optimum of basic knowledge, of degree and extension of standardization of method, and of intensity versus extensity of use of an organism, which it is the task of the investigator to consider and judge wisely. Ordinarily one would risk the statement that the psychobiologist, be he physiologist, psychologist, or sociologist at heart, cannot be too intimately acquainted with the organic type and individual under observation. And the more complex the organism in form, functions, and ecological relations, the more important are intimacy and accuracy of knowledge. It is long before most biologists who attempt to use chimpanzees as experimental objects know enough about them to work intelligently, efficiently, safely, and to their own satisfaction. For knowledge is essential to practical skill and sympathetic rapport, as well as to fruitful observation and safe and otherwise profitable interpretation.

[14] R. M. Yerkes, The significance of chimpanzee-culture for biological research, *Harvey Lectures*, 1936, 57–73.

IV

What from the social standpoint may be designated coöperative behavior is exhibited throughout the animal kingdom: in the invertebrates notably by certain of the insects, and in the vertebrates most strikingly by the primates. This behavior in animals other than man quite commonly is assumed to be racially determined by inheritance or by tradition, and consequently naturally classifiable as primarily instinctive. Individually acquired, variable, adaptable, and admittedly intelligent coöperation, so conspicuously significant in human life and so often singled out as the main hope in man's progress, by many biologists is denied to other organisms. It is one of the purposes of this article to establish its occurrence in chimpanzee and to offer the opinion that it may appear also, although much less frequently and at much lower levels of intellectual integration, in other primates and in certain other mammals.

As social behavior pattern, coöperation certainly is of extreme interest and importance to man. Hence this attempt to help to prepare the way for its experimental analysis, description, modification, and control, in chimpanzee and in other promising subjects of inquiry. Among varieties of social relationship which are commonly exhibited by our apes and which promise to be convenient and fruitful in problem solution, are dominance-subordination; mutual aid, as exemplified in grooming, food-sharing, and parental services; suggestion, and nostalgia. Briefly each will be examined descriptively.

The principle of dominance is universal in the life of the chimpanzee. Every social group presents an empirically established dominance-subordination hierarchy or order which appears to function governmentally. This order is not permanent, but changes with physical condition, age, and the outcome of behavioral competition. Physical prowess and intelligence seem to be its chief determinants. Such a system of social relationship necessitates coöperation, but it does not necessarily imply unselfishness, altruism, nor any semblance of the voluntary subordination of the individual will and desire to the welfare of the group. It seems not improbable that dominance is the basic and all-pervasive principle of chimpanzee socialization and social organization. It asserts itself shortly after birth, and no species-environed individual can escape its operation. Subordination implies compulsory coöperation which often is in the nature of servitude, for the dominant chimpanzee may command of its subordinate the surrender of food, shelter, mate, or whatever is desired. There is, I suspect,

no more promising approach to the comparative, genetic, and experimental study of problems of government than that afforded by the dominance-subordination relations in chimpanzee society.

It would be inexcusable to fail to remark in passing that the successful experimenter or trainer of these great apes of necessity dominates his subjects. Most naturally and fittingly, as well as most advantageously, this is a dominance of wits instead of brawn. For the time inevitably comes when the physical strength of a chimpanzee subject surpasses that of a man, whereas the battle of wits is ever to the latter.

Without apology the reader may be informed that over-great concentration of power or possessions and supreme greediness are as inimical to peaceful social existence among chimpanzees as among men. And although we may not learn much to our immediate practical advantage from intensive study of the nature of chimpanzee social organization, there is obvious justification for using these animals in our search for behavioral principles and ways of producing better types of primate, including the human, and better social relations and social institutions.

Sociological skeptics will not be lacking to question the assertion that mutual aid is exhibited by chimpanzees. Nevertheless, I wish to stand by this interpretation of our observations. Grooming, food-sharing, parental and other adult services to young, injured, ill, or endangered individuals, provide examples of this behavioral category. As I have elsewhere indicated,[15] grooming may be mutual service or it may be wholly one-sided. It may range from assistance in toilet-making to approaches to nursing and surgical aid. In connection therewith, coöperative behavior is extremely common. That it is in any considerable degree altruistic I am not prepared to maintain, but neither should I defend the negative. The reader may be saved serious error of inference if informed that grooming in chimpanzee is much more complex and varied as social behavior pattern than is coat and skin picking (commonly known as "flea picking") in monkeys. Those who know only the latter observationally will find the social service activities of the chimpanzee surprisingly illuminating.

Examples of food-sharing might be selected from our daily observations, but our present purpose is better served by the citation of a special study of this behavioral tendency which has already been re-

[15] R. M. Yerkes, Genetic aspects of grooming, a socially important primate behavior pattern, *J. Social Psychol.*, 4, 1933, 3–25.

ported by Nissen and Crawford.[16] So likewise in the case of mutual aid as exhibited by experimental tests of natural versus acquired coöperative ability, it suffices here to refer the reader to Crawford's detailed report of methods and findings.[17] Such examples of mutual aid, and the lack of it, as these experimental inquiries provide, valuably supplement our daily observational records and supply to a degree the basis for preliminary quantitative statements. For the present we must content ourselves with a dogmatic sounding and wholly qualitative assertion. Fellow service, even where there is no obvious immediate advantage to the agent, is not rare in our colony of apes. Obviously, affective attachment is an important factor. Indeed, liking or affection may be sufficient excuse for what appears to us an altruistic act.

Suggestibility, as social relation, may play a dominant part in conditioning coöperation of the chimpanzee with man or with fellow ape. We rely on it constantly in our daily care of the animals, as also in our attempts to adapt them to their tasks or to train them for a given experimental procedure. They are quick and ordinarily also keen observers of whatever commands their attention, and ever alert to take advantage of cues for individually profitable behavior. Suggestions are constantly seen to gain expression in imitative acts, of which some involve coöperative relations. Probably most apparently imitative activities of the chimpanzee are racially conditioned, and at least primarily instinctive. It would be extremely rash to maintain that understanding or insight commonly functions in behavioral expressions of suggestibility. There is, however, excellent evidence that they sometimes are present and determine the character of the pattern of coöperation. As yet we are at the threshold of the study of chimpanzee suggestibility, and its nature, limitations, and possibilities of exploitation in the experimental analysis of social behavior remain to be discovered. A preliminary report has been published whose results are indicative of the fruitfulness of the topic,[18] and a systematic investigation of imitative capacity, with measurement of the strength of imitative tendency, is being made in these laboratories by Crawford and Spence.

Suggestibility is cited here as a significant type of social relationship, because a non-suggestible animal lacks one of the most obviously useful capacities for profiting by the experiences and activities of its

[16] H. W. Nissen and M. P. Crawford, A preliminary study of food-sharing behavior in young chimpanzees, *J. Comp. Psychol.*, 22, 1936, 383–419.

[17] M. P. Crawford, The coöperative solving of problems by young chimpanzees, *Comp. Psychol. Monog.*, 14, 1937, (no. 2), 1–88.

[18] R. M. Yerkes, Suggestibility in chimpanzee, *J. Social Psychol.*, 5, 1934, 271–282.

fellows. To imitate instinctively may be advantageous, whereas to be able to imitate or to carry out suggestions critically, adaptively, and with understanding and insights, is literally invaluable. The significant measure of suggestibility in chimpanzee favors coöperative behavior and conspicuously enhances the scientific usefulness of the animal.

Yet another social quality or condition which favors coöperativeness in chimpanzee is nostalgia.[19] The animal is highly dependent upon the social factors of its environment, and, eventually in its ontogenesis, becomes, even for a primate, very highly socialized or integrated in a system of individuals and social relations. Because of these characteristics it is constantly impelled to avoid isolation and to seek agreeable or otherwise profitable social contacts. The latter behavior as a rule is facilitated by coöperative behavior. The infant ape, as previously remarked, from birth is dependent upon mother or other adult. In its second year of life, this dependence, greatly lessened, tends to transfer to immature members of the group. Isolation, in the first two or three years of life, of the chimpanzee infant which has been normally and naturally associated with its kind induces acute discomfort, as is indicated by extreme restlessness, crying, screaming, temper tantrums, and varied expressions of fear, resentment, or apprehension. The youngster will do almost anything possible to achieve return to companions. This, we submit, is a behavioral picture of nostalgia. It is impossible to use a nostalgic individual in a behavioral experiment unless it be for the observation of this particular social disorder.

Attempt has not been made to describe inclusively the social relations of chimpanzee which condition coöperation. Instead they have been sampled by brief remark concerning dominance-subordination, grooming, suggestibility, and nostalgia. We would now submit that no organism which commonly and persistently exhibits these relations and their associated behavioral patterns can be non-coöperative; and we would argue further that an animal which is as highly dependent on social stimuli as is the immature chimpanzee is thereby peculiarly qualified for coöperative activity and constitutes preëminently favorable ground for the appearance and growth of unselfishness, mutual aid, and altruism.

To repeat, in these laboratories reliance is being placed increasingly on the voluntary, intelligent coöperation of our chimpanzee subjects. Superficial analysis of the behavioral background and matrix of coöper-

[19] R. M. Yerkes and A. W. Yerkes, Social behavior in infra-human primates, *A Handbook of Social Psychology*, 1935, 1016 ff.

ative behavioral patterns reveals security (freedom from timidity, anxiety, fear), confidence in the social environment, and understanding (intelligent appreciation of the situation), as primary psychobiological conditions of such behavior. Further, we have attempted to prove that such social relationships as are essential to dominance, grooming, and other forms of self and mutual aid, to suggestibility and its forms of imitational expressions, and to nostalgia, are favorable to coöperation, and that its occurrence therefore is implied by their presence in this ape. Finally, it may be argued from these representations, the factual basis for which it is not our present task to present, that the chimpanzee offers unique opportunity for the analytic study of coöperative activity. To this task in comparative psychobiology and sociology, effort will be increasingly devoted.

It remains to consider the status of our knowledge of human coöperativeness. What are its differentia? What is its motivation? How may its appearance and development be furthered? We do not know of definite answers to any of these questions, nor are we able to cite any first-rate working description of human coöperative behavior. There is a surprising dearth of knowledge of these subjects in view of the intensity of interest and widespread conviction of their importance.

At first blush, it might seem that coöperativeness is a variety of behavioral adjustment or adaptation which depends chiefly upon imaginative intelligence and a high degree of socialization. As in chimpanzee, so in man, mental security, confidence, or trust in one's fellows and understanding of the requirements of a situation, are essentials of effective coöperative effort. But man, over chimpanzee, has the incomparable additional advantage of foresight. True, this appears in limited measure in our subjects. Yet even the power of prophecy, however much it may facilitate efficiency of coöperation, would seem to be motivationally ineffective. The crucial question is not how are we enabled to act coöperatively, but why do we do so? For the investigator, chimpanzee motivation has the advantage over our own of being relatively simple, accessible, and amenable to experimental manipulation and controls.

Assuming our agreement that there is nothing sacred about the present psychobiological and sociological characteristics and constitution of chimpanzee, one might reasonably undertake to cultivate unselfishness in this animal and in wide ranges to replace individualistic by coöperative behavioral tendencies. An unselfish ape presumably is quite as difficult to imagine as an unselfish man, and presumably it

would be equally disturbing to the social group, if not also to the established social order! But if we today knew how to render any animal, however different it might be from us, conspicuously socially-minded, we might hope speedily to achieve the basis of solution of some of our most baffling human problems.

Here are some of the questions which the chimpanzee should be asked to help us to answer: How increase unselfishness, and, correspondingly, coöperativeness, genetically? How facilitate and cultivate its development and use? What is the basic relation of selfishness and coöperation? Is our assumption that they are antithetical correct, or do they perchance vary independently? Is unselfishness essential to social-mindedness, and if so, may it be too great or too small? Assuming an optimum degree of selfishness in face of the hypothesis that unselfishness cannot be overdone, how may we best measure the social value of the essential motivational complex and how maintain it at the optimum? Although it is pretty generally believed that man cannot be too honest, too unselfish, or too coöperative for the welfare of his kind, this is by no means self-evident when one critically and without bias examines specific cases. The first difficulty to present itself is that of balance, proportionality, and equilibrium. Too much even of an obviously good thing may prove functionally bad, because unfavorable to other good things.

Thinking thus aloud, we might proceed through the queries and hypotheses, surmises, and biases of generations, for in abundance they are matters of record. It is needless to go further, for what I am trying to express in terms of a study of behavior is already in the minds of millions of men. There is the general conviction, without proper argument or proof, that human nature needs to be transformed to enhance coöperativeness, unselfishness, honesty. That this belief is correct remains to be proved by trial. But before that can become possible, we shall have to learn by patient objective inquiry vastly more than we now know of the facts, conditions, principles, relations, and ways of controlling animal behavior. Surely in view of such extraordinarily enlightening and encouraging experience as we have had in Yale Laboratories of Primate Biology with chimpanzees as material of psychobiological research, it is not extravagant to prophesy that there will presently be written, with the mute assistance of this ape, a chapter in experimental sociology of surpassing natural interest and human value.

PSYCHOLOGICAL REMARKS ON SOME QUESTIONS OF ANTHROPOLOGY

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(Founded in 1921)

In his investigation of primitive mentality Lévy-Bruhl is led to the conclusion that in primitive man mental operations are essentially different from those with which we are familiar. "Les représentations collectives," all the notions with which, during childhood and adolescence, an individual is imbued by the surrounding group, are "mystiques" in the case of primitive tribes. Where we apply the laws of logic "la mentalité prélogique" applies "la loi de participation." The author seems to conclude that in many cases the anthropologist's attempt really to understand and to explain primitive behavior will necessarily end in failure. We even have difficulties in defining the term "mystique." Again, who can formulate clearly what is implied in the "loi de participation"? Still, when we find that one more primitive belief or custom is mystical and that it follows the law of participation this is, according to the French sociologist, perhaps all that we can do about the case in question.

Often, I am afraid, Lévy-Bruhl's scepticism is justified in practice, though perhaps not for the reasons which he indicates. The origin of some tribal beliefs and institutions may be so complicated historically that there simply is no unitary sense in the phenomena themselves. To that extent, of course, we cannot fully "understand" their nature, whether or not primitive mentality differs essentially from our own. Besides, several primitive cultures are known only from reports in which the white observer's preconceived notions seem to be hopelessly intermingled with the ethnological material as such. Not a few of these cultures disintegrate quickly, if they have not actually disappeared. Since the opportunity for more objective investigation is gone, in such cases the available data themselves will remain ambiguous. Much of their obscurity may be due to serious misunderstandings rather than to an alleged impermeable nature of primitive mentality.

Though some anthropologists do not like to admit it, psychological principles play an important rôle in the interpretation of anthropological facts. Theoretical difficulties may, therefore, arise quite as easily from inadequate psychological notions as from the strange ways of primitive mentality. As Lévy-Bruhl has pointed out, the psychology which Tylor and his school applied to anthropological problems can hardly be regarded as an adequate tool in this field. In the meantime, however, psychology has again changed a great deal. Perhaps some of the facts in anthropology which, to the French author, seem to be outside the white man's understanding will prove more approachable if we apply to them recently developed psychological ideas. In the following pages a modest and hesitant attempt will be made in this direction. It must be hesitant because, for the two reasons given in the last paragraph, we should not try to find more psychological sense in our material than it can be expected really to contain. Our attempt must be modest since no outsider can hope to have a sufficient survey of anthropological data for a more comprehensive interpretation of primitive cultures. It ought to be modest in another sense, too, because some of the ideas which I shall bring forward have, at least implicitly, been used before by experts. I should be pleased, however, if psychologists would give some attention to the following discussion. In our desire to be thoroughly exact, we are in the present phase of psychology apt to concentrate our efforts on a few special problems. In consequence of this we begin to lose sight of most questions for which the social sciences expect answers from psychology. If we do not answer because our outlook is too narrow, discontent, of which there are some signs already, will doubtless grow steadily. It may be that the following discussion will satisfy neither psychologists nor anthropologists. In this case it might still have some value, if it induces other psychologists to do better work in the same field.

Many examples may be given in which the behavior of primitive man certainly looks incomprehensible at first. May I select two at random.

Moffat reports from South Africa that once a woman came to him in order to get the medicine which he had prepared for her sick husband. When he made it clear to her that half of the potion should be taken at once and the rest several hours later, she asked whether it would not be possible to drink all of it at once. Upon his assent the woman suddenly swallowed the medicine herself. To the missionary's protest she answered by asking whether it would not cure her husband if she

drank the medicine.[1] Many Africans, it is true, like to take our drugs because of their 'power.' It would, however, hardly be fair to assume that this factor suffices as an explanation. Anthropology knows of too many cases in which what we might call medical treatment or measures of hygiene applies to the family group almost as much as to an individual member upon whom our physicians would concentrate their efforts. Neither is it satisfactory to say that among primitives the individual is less a separate entity than in our society; or that, for them, the individual's boundaries are not the same as for us. Both statements may be true. But in such matters our own point of view seems to us so obvious and necessary that we fail to understand on what basis other views could ever develop.

I have the following report from a missionary who is working in East Africa. While sleeping in his hut a man dreams that he is in another village and that there he commits a crime which, according to the convictions of the tribe, deserves death punishment. Frightened he tells his people about the dream. Unfortunately something of the kind really happens in that other village and approximately at the same time. The man is indicted, he confesses and is killed, although he was miles away from that other village when the crime was committed. Such things will occur less often the more European officials and European schools begin to influence the natives, but my informant, a perfectly calm and sober man, told me that he has direct knowledge of more such cases. Primitive mentality does move in strange ways; it does not seem to mind contradiction. According to our thinking the man could not be in another village while he was sleeping in his hut. This is exactly, however, what the natives believe and what the poor victim of such logic apparently believes himself.

I have gradually become convinced that in many such cases our difficulties are due to a special coördinate system of our own no less than to peculiarities of primitive mentality. In a way scientifically trained people of our time may be particularly unfit for the understanding of less intellectual cultures. What are the objective facts of nature? What is the best access to objective knowledge in this sense? What influences on the other hand are apt to hamper our progress in this field? Since the 17th century such questions have gradually introduced a definite set of values which is now so dominant that far beyond the circle of scientists proper the outlook of civilized people is thoroughly

[1] R. Moffat, *Missionary Labours and Scenes in South Africa*, 1842, 591 f.

governed by these particular ideals. A sober attitude towards a real world is instilled in children of our civilization by the words and actions of their parents. Long ago the most basic convictions of scientific culture lost the character of theoretically formulated sentences. Gradually they have become aspects of the world as we *perceive* it; the world *looks* today what our forefathers learned to say about it; we act and we speak accordingly. In this form the consequences of a few centuries of science are present in the remotest corners of the civilized world.

The ideals which I mentioned, the special interest which they represent, and the particular aspect which they have given to the world, are doubtless very apt to further quick progress of natural science and practical behavior in contact with reality. I wonder whether to some degree they do not disturb the advance of anthropology. Physicists have gradually separated their real world from the realm of mere percepts. In doing this they have also learned to distinguish between phenomena which tell the truth about the real world and others which are illusory. On this basis percepts and their traits are good or valueless according to this distinction. Similarly, properties of percepts which are easily understood in their relation to physical stimulation will be readily accepted in sense physiology. Whatever does not fit into this scheme will be neglected and despised because of its 'subjective' nature. Under the influence of scientific culture the very appearance of the world around us in being transformed. It will not only be 'intellectualized.' Such characteristics of the world will become less conspicuous, they will gradually recede into the background, for which the physicist and the sense-physiologist have definitely no use. Those aspects on the other hand are made outstanding which we regard as representatives of reality.

For natural science, I repeat, this development may mean strict progress. I shall not deny that the modern aspect of the world around us agrees better with our picture of physical reality than older views did. But I insist upon the fact that before the era of scientific culture the perceptual world could not have looked as it appears to us now. Psychologically speaking our view of the 'given' world is the product of complicated historical processes. Judged from a merely psychological standpoint and without any reference to physical reality this aspect of the world is therefore no more genuine than any previous appearance of man's perceived environment. To some extent even more 'natural' traits of perception will be found where science has not exerted its

modifying influence. They would be most prominent in the primitive's world.

Their study, however, is difficult. The psychologist no less than other people has learned to see the world through the eye-glasses of natural science. What he beholds in this manner seems to him so obvious, he is so little aware of his eye-glasses, that any other view will appear to him as a distortion of the genuine aspect of the world. If primitives have such another view, their mystical thinking will be made responsible for it. They have changed the genuine appearance of the world which is, of course, originally the same everywhere and identical with our own view.

It seems to me that the opposite judgment would be at least as correct. Primitive perception, to be sure, contains a great deal of intellectual elaboration. It is improbable, however, that such influences should ever have been more systematic and intense than have been the effects of scientific learning in our own case. It would therefore be of great advantage if, in studying the appearance which the world has for primitives, we could overcome the tendency to use our own view as a norm. Anthropologists have generally been at pains to follow this principle. More might be done, however, if even in the application of psychological thought to primitive perception the white man's customary outlook were recognized as a disturbing factor. Recently we learned that a definite type of scientific analysis had nearly made us unable to apprehend most common sides of perception and of mental life in general. We shall try to apply this lesson to anthropological questions.

Almost 50 years ago Von Ehrenfels pointed out that percepts have properties which cannot be derived from so-called sensations. A sensation has a good physiological standing. It is the phenomenal correlate of a well-defined stimulus. The properties of percepts which Von Ehrenfels had in mind seem, however, to occur only where groups of stimuli coöperate in causing one phenomenal datum. The stimuli in question may be spatially or temporally distributed. Correspondingly, Von Ehrenfels' qualities are the attributes of percepts which extend through areas in space or through stretches of time. They are not popular in sense-physiology, which likes to study the reactions of single receptor-cells to single stimuli. Properties which are characteristic of larger entities—and only of these—will evidently not fit into this scheme. A well-known example is the 'major' or the 'minor' character of a melody or of a chord. No single tone has minor character, whereas it is an essential trait of many tunes and chords. Although

quite familiar to musicians, among psychologists such traits of auditory percepts had never before found the attention which they deserve. After what has been said above we need not discuss the reasons for such aloofness.

It would not be difficult to show that all percepts have such qualities. When things are called 'tall' or 'bulky,' persons 'slender' or 'stout,' movements 'clumsy' or 'graceful,' reference is made to definite Von Ehrenfels-qualities. When we describe events as 'sudden' or 'smooth,' 'jerky' or 'continuous' we refer to the same class. Esthetically they are doubtless of paramount importance, but they are hardly less so, I think, in biological contexts. The color of the hair, the pitch of the voice may have much to do with the strong impression which specific persons of one sex make upon specific persons of the other. Still, properties of shape, of gesture, and of general movement are generally found to be at least as dangerous—and these are Von Ehrenfels-qualities.

In a certain sense such qualities of percepts are 'subjective.' As we perceive them they are functions of nervous processes; but this is true of all properties of all percepts. They seem more 'subjective' than 'green' or 'blue' because no simple physical stimuli are responsible for their occurrence. It may be doubted whether, despite this serious disadvantage, they are not often particularly important as witnesses of objective reality.[2] Beyond any doubt they appear quite as much as properties of the world around us as, for instance, colors and smells. Phenomenally, they are *objective*.

If I am present, when two cars crash at a crossing, and if then I speak about the 'vehement' or 'violent' impact of one upon the other, one might say that in this description I attribute to what I see traits which are borrowed from my own muscular or other subjective experiences. My answer would be that this is not the point which we are discussing at present. Whether the explanation by 'empathy' is right or wrong, we have to distinguish between the functional genesis of phenomenal characters and their phenomenal appearance and localization as such. Supposing even that Von Ehrenfels-qualities like 'vehement' have a subjective origin, from a descriptive point of view they are therefore no less objectively localized, are no less aspects of events themselves. Clumsy dancing looks clumsy there, outside, in a man or woman whom I see moving before me. It is again Fred Astaire, not I, who performs unheard-of Von Ehrenfels-qualities in dancing; it is on the screen that I perceive them, not in my muscles. If genetically I should be responsible

[2] Wolfgang Köhler, *Gestalt Psychology*, 1929, 174 ff.

for all this, I am at least in simple experience unaware of my authorship. After all, it is a theory, not an observed fact, that such traits of percepts are always due to subjective sources. For unsophisticated perception vast numbers of Von Ehrenfels-qualities appear precisely where colors and other attributes appear with which customary thinking is better acquainted: in and on things, persons and events. It follows that the less sophisticated people are, the more will they accept such properties of percepts at their face value.

Primitives are, of course, naïve realists. They cannot possibly distinguish between 'mere percepts' and independent physical realities. A thing-percept is for them the very prototype of reality, although they believe in further realities besides. In fact, percepts are for them—as for children—so real that they would hesitate to accept the name, if they could clearly understand its meaning. Why should they call a tree a 'percept'? There simply *is* a tree before them to which, it is true, more or less attention may be given. That this tree which we call a percept remains unaltered in its place even when nobody is present who might have the percept, this is, for a naïve view of the world, the most natural thing. When before us, the tree certainly shows no striking symptoms of depending upon any subjects for its existence. One has to be far ahead in physics, physiology, or philosophy before anything like a problem can be discovered in this connection.

It is again well known that without such sophistication no doubt will ever arise about the independent reality of all the sensory qualities which the tree or any other object may possess. We have seen, however, that the Von Ehrenfels-qualities of percepts are localized in and on these percepts. Primitives have no physics, no physiology, and no epistemology which could throw any shadow of suspicion upon these qualities. Consequently their naïve realism will apply to such characteristics of the world as it applies to any others. For them, things and events will have these properties independently and objectively. Thus their environment will have many aspects to which they attribute full objective value, while our scientific civilization denies such value and accordingly tends to impoverish our perception.

Many of these qualities are distinctly *dynamic*. What is 'sudden' may, with increasing intensity, become 'startling,' frightening' and 'threatening.' Such terms imply the subject's emotional reaction. But they also imply that in certain events or things there is a perceptual basis for the reaction. Here, as so often, much phenomenological truth is contained in language.

Not always is the 'threatening' a high degree of the 'sudden.' While climbing once in the Alps I beheld, on stepping cautiously around a corner of the rocks, a big dark cloud which moved slowly and silently towards me along the slope. Nothing could look more sinister and more threatening. Genetically this might have been a case of empathy; but for my awareness the menace was certainly in the cloud. I could perhaps persuade myself that a cloud as such is an indifferent percept. If, however, I had been a primitive, no reason whatsoever could have given me such sober consolation. The threatening character of the cloud itself would have remained just as 'objective' as its ugly dark color. Similarly the ocean itself is 'wild' in a gale; and a mountain which appears high above the other tops in strange illumination is itself 'majestic' or 'forbidding.' Primitives have no science of physics in our sense of the word, but those opinions about the world which they develop instead will necessarily differ widely from our own views. If the expression is admissible, it must be a curious 'physics' in which the menacing character of a cloud, the wildness of a gale, the majesty of a mountain-percept have at least the same rank and objectivity as, with us, measured distances, velocities, and weights. It is in the nature of such 'physics' that it can never be a calm and critical survey of facts as our science likes to be. The primitive's reality is apt to be too exciting for sober appreciation, since not only its effect on the subject, but also its own appearance is so often emotional. Awe and respect are more likely to characterize the primitive's attitude in such matters than is a spirit of critical investigation. Whatever else may follow, from this starting point ways may even lead to religion.

In passing, a remark should be made on an old philosophical controversy. David Hume denies that any of our 'impressions' contain the quality of power or force. His opinion can only be held so long as, in a quiet environment, we discuss phenomenological questions in abstracto. At his writing-desk the philosopher is apt to see the world as static and tranquil. It is, however, the sailor in a small ocean-going vessel, the airplane-pilot, the tramp left to his own resources in any weather, who can tell us best whether there are any experiences of power, force, or dynamic traits in general. For us it is, of course, another question whether such dynamic traits of experience have any physical counterparts; but on the level of naïve realism this question has no sense. Here the objective world exhibits forces directly and not at all infrequently.

Not all dynamic qualities of things are startling. There is 'heaviness' in a middle-sized stone, 'hardness' in the same object, 'elasticity'—not as a property defined by the physicist but as a perceived quality—in a young tree which my arm bends. Everyday physics of the layman deals with such properties of things as he is directly aware of them.

This is true in Central Africa, but also in civilized countries, whenever people do not actually use the more sophisticated concepts of physical science—which is of course only done by a few. Regularly and with many objects this procedure is quite safe, because in many respects such simple Von Ehrenfels qualities correspond fairly well with certain concepts of physics. So long as this is the case nobody objects and nobody would speak of mystical ideas.

Now and again, however, the situation acquires a slightly different character; namely, where the dynamic side of things and events grows beyond our feeling of control. Instead of a young sapling a primitive may have bent an older and stronger tree. If he had any physics, he would look upon the situation with some knowledge of objective danger, otherwise with a sober and calm mind—but he has no physics in our meaning of the word. Instead, there is now in the tree a perceived power beyond the limits of what the man is able to handle safely. He will respect such power. It may be actually frightening when suddenly the tall stem breaks its bonds and jumps fiercely up into space. There is an awe-inspiring quality in the thing, just as there is in the sudden rush of high wind which often precedes a thunderstorm, or in the lightning which accompanies it.

Even when enhanced beyond the common and concentrated to fearful intensity, such characteristics will for naïve realism still remain objective properties of things and events themselves. There is no reason why primitive man should not behave correspondingly. From the more trivial power-qualities a nearly continuous series will extend for him through more striking cases to those where he is overwhelmed by fear. It is only natural that in his environment nothing should appear to him more important and impressive than unusual power in this sense, particularly since he knows so little of its rules and whims.

Here, it seems to me, we have the perceptual foundation of what some Melanesians call "mana," the Crow Indians of North America "maxpé," and some tribes in West Africa "njomm."[3] Mana at least is a notion in which the thinking of widely differing peoples seems to agree. When Codrington first mentioned it, it appeared as a rather enigmatic term. If the phenomenological side of psychology had then been further developed the notion would have seemed less puzzling. In earlier phases of anthropology, before mana became better known, the notion was probably often misunderstood to mean a deity, a spirit, or a soul in things. Though it seems possible that on a certain level the

[3] R. H. Lowie, *Primitive Religion*, 1924, 75 f.

concept of a soul develops as a special case of the general class mana this term and its synonyms cannot originally and generally have had the special meaning of our word 'soul.' Tribes in many parts of the world find mana in certain things and events; but among these tribes there are some in which not even the human person himself has a soul comparable in meaning with the soul of Christian doctrine or Descartes' philosophy.[4] Few factors in our civilization make the anthropologist's work so difficult as our popularized mind-body dualism.

I should also hesitate to interpret mana as though it meant 'supernatural.' Where a thing, a person or an event has mana it remains for primitive man on the same general level of existence as other parts of his environment, although it is more conspicuous by its peculiar appearance or inherent power. Using our word "supernatural" in this connection we are apt to lose sight of those obvious phenomenal facts upon which, if I am right, the notion of mana is ultimately founded. To some extent all things may have their mana. Innocent enough as simple heaviness of a stone, it may still be the same in principle where its intensity or particular behavior arouses respect and fear.

I mentioned above that in ordinary life our own treatment of many things refers to their power-qualities as we perceive them naïvely. This attitude, however, is rather strictly limited by the scientific atmosphere in which we live. We have learned that most mana is merely a 'subjective' phenomenon. No such restraint disturbs the naïve realism of primitive man. He remains consistent in handling things according to their inherent dynamic qualities. Consequently some of his activities will seem to us quite natural and sober, others altogether strange because they are contrary to the lessons of science. Here we begin to speak about *mystical* thinking and about *magic*. Westermann, however, denies that for the Kpelle themselves there is any sharp dividing line between 'normal' activities and everyday magic. This may not apply to the Indians of North America; but among Africanists the same opinion seems to win more and more adherents. Take the special case of medical practices. "The word *musamo* . . . connotes . . . not only various medicinal remedies proper, but also, and much more, many things whose power we should call magical. The difficulty is to separate the two. From the native point of view there is no difference."[5] When the African takes a hearty meal and thereby "fills himself with power," we are not surprised. If he is convinced, however,

[4] D. Westermann, *Die Kpelle: Ein Negerstamm in Liberia*, 1921, 174 ff.
[5] E. W. Smith and A. M. Dale, *The Ila-Speaking Peoples of Northern Rhodesia*, I, 1920, 221.

that by eating the meat of a courageous animal he will himself acquire more courage, we speak about superstition. As a matter of fact, we have no evidence that for the native there is any essential difference between these cases.

With this example, it is true, we have touched upon a new aspect of power-qualities. Even where for us their functional subjectivity seems altogether obvious, people without any science will still remain naïve realists. For an object to have mana it is by no means necessary that its peculiar characteristic appear under all circumstances. The *new* thing, an event which does not usually happen, is for this extraneous reason apt to be mana. Certainly, the unexpected looks different; it has its peculiar appearance even for us; surprise, as other emotions, has its phenomenally objective counterpart. But we subtract easily from "the real facts" what, for our critical attitude, reveals its subjective genesis so easily. Not so the native. Objects may have for him the very highest degrees of mana which are for us as trivial as possible. He does not yet realize clearly that the 'striking' in the new thing is due to its newness only. Strangeness is a property of the thing itself; awe is the consequence, and action will be taken correspondingly.

On the other hand, if an object has once exhibited power, such experience has an after-effect. It will look its power or have the flavor of it, even under conditions where, without such previous experience, nobody would find its mana-aspect very impressive. If in our own psychology we had no examples of "sign-gestalt," as Tolman calls it, we could hardly understand much primitive behavior. It is this principle which imbues thousands of objects with power which for the neutral observer may seem quite incapable of having any particular mana. When the courageous animal has been killed, its meat certainly does not look courageous to those who do not know from what it has been taken. But as the living animal will be regarded as courageous even at times when it is peacefully at rest, so its meat will still contain the valuable power. We add sceptically: for those whose apprehension of the object is distorted under the influence of previous experience. Yet the natives are not so sophisticated. So long as naïve realism is quite undisturbed the effect of previous experience prevails, while little attention is given to its psychological history. After all, it can happen here. The place where yesterday a cruel murder was committed would have today a strange look for most of us, even if no visible trace of the crime were left; many would feel uneasy in such a locality, and few would choose it as their living-place.

For the native mana-qualities do not only *exist* in his environment, they also have definite forms of *behavior*. At least one of these is easily understood, if only we begin with examples where our own judgment agrees with that of primitive man. What is a smell? Very few people, those who have studied this special part of sense-physiology, can give a clear answer. On the level of naïve realism on which to some degree we all remain in everyday life a smell is a quality that tends to spread from one object and one place to others, as though it had the properties of an intangible light liquid. We do not put kerosene near butter or bread, lest the kerosene smell penetrate into the food. With onions the same precaution has to be taken, although here it is another quality which spreads to surrounding objects.

Smell is by no means the only entity that shows this behavior. From a fire the quality of heat migrates in all directions. The rain, the fog and the thunderstorm spread from one place to the other. Most important of all, many a sickness creeps through the community from hut to hut; and an uncanny change which has just come over one man is soon discernible in others. Qualities of all kinds seem thus to travel through space, even though we cannot trace their ways in every detail. Should we object to this description of facts? One hundred years ago heat was an unweighable stuff even in orthodox physics; and its behavior was akin to that of smells and of infectious diseases. In a more general formulation: Qualities are transmitted from one object to another; and for primitive thinking such *transmission* seems to be the basic form of *causation*.

The same idea lingers still in our thought. It may also be entitled to more attention than it receives in modern epistemology. Even now our theorists are under the spell of David Hume's powerful work. Transmission of qualities has no place in our positivistic concept of causality. For our present purpose this concept may be sufficiently characterized as follows: An object has the property x, while in the same object and in its immediate environment we have the conditions $a, b, c \ldots$. When $a, b, c \ldots$ change into $a^1, b^1, c^1 \ldots$, instead of x the object assumes the property y. It is, since Hume, customary to say that no internal relation connects the nature of the change $x \rightarrow y$ with the nature of the change $a, b, c \ldots \rightarrow a^1, b^1, c^1 \ldots$ Facts of coexistence and of sequence are *mere* facts, which means that a priori any datum might coexist with or follow any other. In principle, we do not get more out of nature than incomprehensible rules.

One sees at once that between this idea of causation and causation

by transmission there is a striking difference. According to the positivistic notion, no matter how long we consider causes, we can never derive from this inspection any prediction as to the nature of the effect. Contrariwise, if causation is transmission of an identical quality, state or entity from one locality, it follows necessarily that the effect, the appearance of that entity in the second place, must be similar to the cause which is the same entity as it appeared in its first place. *Causa aequat effectum.*

It seems appropriate at this point to say one more word about a famous period in the history of European philosophy. Why should Descartes' school be so uneasy about the idea that mind and body interact? The reason seems to be simple. In his time causation was still widely conceived of as transmission of some quality from one thing to another. This primitive but rather plausible idea had much power on the best minds as late as 1650. Two incomparable substances, they say, cannot influence each other. A modern positivist might ask: Where is the difficulty? But for the older view the problem is doubtless serious. If mind and body are really incomparable substances, nothing can be transmitted from one to the other. Otherwise the second would, by the fact of transmission, become similar to the first, which is impossible, since they are supposed to be incomparable for ever. Thus neither can exert an influence upon the other. "Influence" is just the right word. No other term could express more characteristically what causation normally meant before the concept was so utterly changed by Hume.

Primitive people could point to many examples in which observation agrees well with the concept of causation by transmission. Effects are so often similar to causes that modern epistemology would do well to give the fact at least some attention. Take any of the cases which I have mentioned above. What is first in one place soon appears as something similar in the neighborhood. Some qualities do travel like thin fluids. But primitive man never formulates a definite notion of causality. It is probably not by a great number of confirming experiences that he is induced implicitly to favor just this particular notion. Rather its intrinsic plausibility seems to make it dominant—so dominant in fact that where the primitive is definitely wrong the white man's appeal to contradictory experience is generally of no avail.

Very often the primitive *is* wrong. Unacquainted with any of our critical procedures he extends his naïve realism to all possible qualities of things. Quite as uncritically he applies to all these qualities his unformulated principle of causation by transmission. We accept some of the qualities and neglect many in our practical behavior; for we are trained by science. We are not very clear about causality; but our tacit acknowledgment of causation by transmission is definitely checked, again by the scientific attitude of our culture. Superadded criticism

distinguishes our view of the world from that of primitive man, not mental traits of his which we could not understand at all. Once more: It seems a misconception to say that in some domains primitive man acts soberly, as we do, while in others he relies on peculiar mystical practices which constitute the sphere of magic. So long as no scientific criticism is possible the practices of magic are probably as natural as any activities of which we approve. Both spring from the same sources: naïve realism and causation by transmission.

The fundamental rule of magic follows from these premises. If you wish to produce a definite effect in an object or in a person, you have to bring near him or, better, into him such things as are conspicuous by the quality in question. When we are cold we approach the fire or take a hot drink. This is not regarded as magic. Apply the principle generally and civilized people will say that you perform magical practices.— The primitive's child is slow in acquiring speech. The child's tongue lacks the necessary agility. Where do the parents find agility? In the lizard's tongue. Consequently the child will be fed lizard's tongues. The white critic might say that agility is not like calories which can really be transmitted from one object to another. Why not? The strange power of fire can thus be transmitted. We light a match, hold it near the cigarette and at once the burning quality is transferred to the cigarette. For people who have no science there is no essential difference between one case and the other. Among the Thonga in South-East Africa a mother who has not enough milk for her baby will eat euphorbia plants. These contain the necessary milkiness.[6] Without any chemistry people will rely on perceptual characteristics as they are directly given. Among the Spaniards of Tenerife, men who are losing their hair will eat the hairy stems of certain plants. The 'hairiness' of these will make the hair grow. Primitive mentality need certainly not be essentially different from our own in order to develop such ideas. It has only to remain naïve and, in its basic primitive conceptions, thoroughly consistent.

Where power-qualities are not absorbed in the form of food, but supposed to spread through space, magic seems to us more strangely magic. Still, the underlying principle remains the same and agrees with our experience in the case of smell and heat. Instead of eating the strongest parts of a strong animal a person may wear them somewhere on his body. From this *amulet* the power-quality will permeate the owner. Since there are many capacities which a rich man will wish to possess

[6] H. Junod, *The Life of a South African Tribe*, 1927, 47 ff.

he may be forced to carry along a bundle of different power-substances, one for each purpose—as he really does in West Africa. Contact is, of course, better than mere neighborhood. A few years ago they had the first national sweepstake in a country of Western Europe. A sailor won the big prize. When he came to the capital in order to get his money, the girls in the office crowded around the man and touched his body with their fingers. Apparently they felt that some of his luck-power might be conducted to themselves.

For reasons which I have mentioned above considerable power may be contained in an object which, to European eyes, does not look conspicuous. This is generally true of those substances which are the main parts of so-called fetiches in West Africa. It is widely recognized at present that as a rule these objects are not idols or images of gods, but mana-batteries like our storage-cells. Their preparation by special experts involves the same principle of causation by transmission. In a special hut, protected against disturbing influences, the prospective new fetich is put near an old powerful battery and thus gradually charged.[7] It is in line with such ideas that when wrongly treated even the best fetich may lose much of its power. If that happens it is recharged or sold at a lower rate.

Even 'imitative' magic, at first one of the more surprising activities of primitive man, seems to follow from the same principles: naïve realism in an extreme form and causation by transmission. At the time when rain should fall certain Mexican Indians carry a baby around the fields. The child is known to contain the power of nearly inexhaustible wetness. Apparently this quality will spread and soon it will begin to rain. I hope I do not exaggerate if I say that most monographs on primitive peoples contain reports about analogous practices. Often imitative magic produces the characteristics of an event rather than of any object. We know of several tribes where lightning or thunder is imitated as representative of thunderstorms and heavy rains.

I am not overlooking the fact that definite formulae and chants are generally quite as essential for magical purposes as powerful objects and performances. What can mere language have to do with mana? The obvious answer is that for naïve perception language may contain at least as much mana as any objects; and certainly it spreads. Once more our lack of understanding is self-produced. We know about sound waves, we have learned that in language indifferent auditory phenomena are merely associated with meanings. Such knowledge

[7] E. Pechuel-Loesche, *Volkskunde von Loango*, 1907, 366.

makes us unfit to realize what language is for the unsophisticated mind: a stream of often forceful content that issues from the mouth of man, full of hatred in one case, of love in another, of despair in a third. How could primitive people come upon the theory that language consists of sounds to which the hearer adds a subjective interpretation? Whatever may be true functionally, from a phenomenological point of view even we must acknowledge that this is not a correct description of language, that for naïve apprehension meanings are *in* the words and sentences of common speech. Small wonder that the native's naïve realism remains consistent in this case.

May I take an example from one of Malinowski's books. When among the Trobriand people a woman expects a baby much has to be done for the child's health and beauty. One practice is this: Other women prepare a special skirt or coat for the mother. During their work definite formulae, sweet words about beauty, are spoken literally into the coat. Carefully folded, so that the wind cannot reach the inside and thus remove the charm, the cloth is carried to the mother who, with the coat, covers herself with the mana of those words.

In anthropological literature similar ideas are even applied to questions of human personality. Phenomenologically this seems a sound procedure. Some persons have a strikingly powerful appearance; and many people are conspicuous for their particular varieties of perceptible personality-mana. When we think of unusual people, perhaps for a moment only, of Mussolini, of Lloyd George, of Greta Garbo, there is before us, often with a visual image, a characteristic and distinctive quality of being or living in each case. We cannot define it, yet it is obvious enough; it has the earmarks of a Von Ehrenfels-quality, comparable with the Mozart-character of some music and the Brahms-quality of another.

Since even common people talk and act in their individual manner, in all of them there is *some* mana which gives them or, perhaps, *is* their particular kind of aliveness. At certain times, however, this power does not show so clearly; apparently it is absent. This would not be more surprising than that heat or a smell are here now and elsewhere later. Take the case of the man who is sleeping in his hut. Compared with what he is in day-time, he seems to be completely lacking in his distinctive personal essence. In the morning when he awakens it will be evident again. Where, then, has it been in the meantime? The man had a dream in which 'he' was in another village. Thus we see where his personal power roamed while his body lay inert in the hut. When

temporarily outside the body, that power will of course behave as human persons do. The dream tells us about its concrete activities. If these were of a criminal nature, punishment will seem to be fully justified. Lévy-Bruhl assumes that for native belief a man may be in two places at the same time, and that we can do no more than accept such an extraordinary contention. I hesitate to adopt this sceptical attitude so long as, in the manner just indicated, a more plausible interpretation is available.

Is this one more attempt to offer a standard solution for all anthropological enigmas? It is not meant to be. Criticizing another thesis, Lowie has once said, "that its very simplicity should militate against its acceptance."[8] I admit that the principles which I have tried to apply to primitive behavior are simple. But, then, only the very simplest forms of such behavior have been given an interpretation; and the discussion ends at a point where both problems and possible explanations begin to assume a higher degree of complexity. Hardly a word has been said about primitive religion, none about primitive art, again none about social life with all its institutions and ramifications. I am convinced that in these fields, too, psychology can be of more help to anthropology now than, say, thirty or even twenty years ago. But for such help other principles besides those of the present essay will have to be introduced. Even the problems which have been treated present certain aspects which can hardly be understood on such a narrow psychological basis. Causation by transmission, for example, is doubtless an essential factor in primitive thinking. But we know many primitive beliefs which, although somewhat similar to those discussed above, can nevertheless not be reduced to this principle. Lévy-Bruhl is right, I believe, in his assumption that typical cases of "participation" do not involve what we should call causation in any of its forms.

A more serious objection might be raised precisely on the ground that primitive society as such and its overwhelming influence on the individual have not been mentioned at all. No special form of magic, for instance, to which I have referred should, according to this criticism, be interpreted in terms of 'general' psychology. Only in the group, as products of its creative faculty, can such practices ever arise; to the group the individual owes his magical beliefs; and as the nature of group-life varies from one part of the world to another, the importance of magic and its special forms vary correspondingly.

[8] R. H. Lowie, *Primitive Religion*, 1924, 259.

This objection, it seems to me, is partly justified insofar as no attempt has really been made to explain any differences which, in such matters, are found between the various peoples of the globe. These differences are remarkable and of the greatest interest for anthropology. The reason why they have not been discussed it quite simple. One cannot do it without at once becoming involved in all the major disputes of present anthropology. On the other hand such a discussion was not strictly necessary. To some extent magic exists in practically every society. If my interpretation is right, at least some of its major premises are not peculiar to a few specific tribes but are the common property of all mankind below a certain high level of sophistication. From this common stock, which general developmental psychology is entitled to study and to explain, different societies, with their different environments and histories, have in fact derived different varieties of actual practices. I do not believe that we can fully understand the origin of such varieties before we know on what ground magic in general grows. That this ground is the group as such and that the individual is no more than an empty container for the products of group mentality, seems to me an unacceptable thesis. If the individual were intrinsically incapable of such attitudes as fear, respect, aversion, love and joy, of such functions as perception, learning and memory, no influences of the group, however strong, could ever proproduce in him these mental events. It is my contention that the same applies to the psychological sources of magic. What springs from these sources is codetermined by the particular tradition of the group. Much magic also applies to group-relationships as such, but the group contains no ultimate source of magic which is not also found in the individual.

THE PERCEPTION OF SPACE BY MEANS OF PURE SENSATIONS OF TOUCH

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(Founded in 1911)

Tactual space has long been regarded as entirely different from visual space, although a sufficient knowledge of the spatial properties of stimuli may be acquired by means of tacto-kinesthetic sensations only, as usually happens among blind people. As a matter of fact, laws by which sensorial data are organized as spatial configurations appear to be similar both in vision and in touch. Spatial qualities such as length, breadth, height, nearness, and distance are common to both senses.

On the other hand, researches which have been carried on at the Institute of Psychology of Turin (Italy) show that the perception of visual stimuli undergoes remarkable changes, if these are being looked at while making muscular efforts.[1] Working along the same line, Werner has shown that there are some intersensorial qualities—such as brightness—which are common to all senses.[2] Werner, in order to explain this fact, admits a primordial sense to which is referred the later development of the others. If this be true, it might be said that, from a psychological point of view there are not as many spaces as there are senses, but only *one* perception of space, the outward appearance of which varies according to the sense referred to. As to vision and touch, it may also be said that, while we do not require sight to get even a good knowledge of space, this latter when it is deprived of any notion of movement has no meaning.

As a matter of fact, the investigations which have been carried on by myself and by my students point to the fact that the perception of length and rectilinearity varies both in vision and in touch, but follows the same law of variation.

The difference-threshold for the perception of linear stimuli in vision

[1] Angiola Costa, Influsso di sensazioni cinestetiche su concomitanti percezioni visive, *Arch. ital. Psicol.*, 13, 1935, 135-173.
[2] H. Werner, L'unité des sens, *J. Psychol. norm. path.*, 31, 1934, 190-205.

has been investigated by Kiesow by means of very accurate researches.[3] By comparing stimuli of different length Kiesow found that the minimum amount a straight line has to be increased or decreased to perceive its change, is proportional to the length of the line itself. Kiesow's results are given in the following table. R means the length in mm. of the stimuli employed, and $\Delta\tau/R$ the fraction between the mean difference-threshold and the single stimuli.

TABLE I

KIESOW'S RESULTS FOR THE DIFFERENCE-THRESHOLD OF LINEAR EXTENTS IN VISION

Stimuli	$\Delta\tau/R$	Stimuli	$\Delta\tau/R$
10	0.010500	80	0.008675
20	0.009900	90	0.009522
30	0.010330	100	0.008510
40	0.010450	150	0.008360
50	0.009200	200	0.006530
60	0.008400	250	0.007724
70	0.009286	300	0.006573

These results show that the difference-threshold is proportional to the length of the stimuli. As a matter of fact the ratio between the difference-threshold and the stimuli approaches a constant value from at least 10–150 mm. stimuli (see Table I). In other words, Weber's law has its full meaning when we compare the extent of two visual stimuli. We may express this fact in the equation: $y = f(x) = ax$; where y means the absolute difference-limen, x the stimulus and a is a constant.

I have myself observed a similar law as regards the difference-threshold of rectilinearity in the alignment of three punctiform visual stimuli.[4] By means of a special apparatus, one of three movable ink-spots of 0.25 mm. radius, put on a straight line at the same distance one from the other, can be moved by minimum steps until an angle is first perceived between the movable spot and the others. This minimum angle may be taken as the difference-threshold of rectilinearity. The results of 2400 determinations show that the angle remains the same, whatever may be the direction of the spots and the distance between them.

As to the subject *A.G.*, for instance, the average threshold amounts to 2.19714°, the mean variation being equal to 0.258585.° The average is taken from the values obtained for 5 stimuli, the spots being placed at the distances of 20, 30, 40, 50, and 60 mm. As the minimum angle

[3] Federico Kiesow, Über die Vergleichung linearer Strecken und ihre Beziehung zum Weberschen Gesetze, *Arch. f. d. ges. Psychol.*, 56, 1926, 421-451.

[4] Alessandro Gatti, Ricerche sperimentali sopra la soglia di rettilineità, *Arch. ital. Psicol.* 11, 1933, 89-112.

which is perceived in the alignment of visual punctiform stimuli is equal to a constant, we may write: $\epsilon/d = tg\alpha = k$ where ϵ means how much the spot has to be moved to perceive the angle, d the distance between the stimuli, α the mean minimum angle perceived by each subject, k a constant. In other words, the difference-limen of rectilinearity, measured by the linear distance which one stimulus has to be moved in regard to the others, is proportional to the distance between the stimuli. The problem arising from what I have said may be stated as follows: to what extent does the difference-threshold vary in the so-called tactual space with regard to length and rectilinearity?

As is well known, spacial properties of stimuli may be acquired apart from sight, by a twofold process. First, stimuli may be held in the hands and touched by moving the fingers all around them. In this case one cannot speak of pure sensations of touch, but of tacto-kinesthetic perception (*aktive Tastwahrnehmungen*). In the second case, stimuli may be put, or even pressed, upon the skin without moving them. This latter process is most unusual even among blind people, and does not allow a good knowledge of space; but for this same reason it appears to be most convenient to our purpose, as it is further removed from our usual perception of space by means of sight. This is why we have carried on our researches in the field of pure sensations of touch (*passive Tastwahrnehmungen*).

The purpose of our investigation has been to find out the difference-threshold for the perception of length and of rectilinearity of stimuli pressed upon the skin. As stimuli we have employed first, card-board edges, then, three, and lastly two pointed stimuli in a straight line. The stimuli were placed on the skin by means of two special apparatus in order to keep the same pressure on the skin and to allow minimum changes in the length of and in the angle between the stimuli.

As to the difference-threshold in linear stimulation of the skin, Dr. A. Danesino,[5] one of my students, carried on three different researches, using cardboard edges of 10, 20, 30, 40, and 50 mm. length for the first research, and punctiform stimuli of two and three points for the second and third. As to the psychophysical procedure, I suggested the minimum variations method. The area of the skin stimulated was kept the

[5] Angelo Danesino, Sopra l'apprezzamento di differenze spaziali nel campo delle sensazioni tattili pure, *Arch. ital. Psicol.*, 10, 1932, 160-167; Nuove ricerche sopra l'apprezzamento di differenze spaziali nel campo delle sensazioni tattili puro, *ibid.*, 11, 1933, 151-156; La soglia di differenza nel confronto di tratti spaziali nel tatto puro, *ibid.*, 14, 1936, 232-239.

same throughout all the researches, viz., the middle point of the left inner fore-arm.

The results of Danesino's careful investigation show that the absolute difference-limen necessary to perceive the increased or decreased length of stimuli does not vary, no matter what may be the length of the stimuli. This astounding result was such a surprise to me, that I wished to repeat the research myself on two different areas of the skin; namely, the middle point of the left inner fore-arm and the *glabella frontalis*. I used Danesino's punctiform stimuli of 2 and 3 points.[6]

I give in Table II all the values of Δr (absolute difference-limen) as they result from my own and Danesino's studies.

TABLE II
COMPARISON OF RESULTS OF DANESINO'S AND GATTI'S STUDIES

		Length of stimuli (in mm.)				
	Study	10	20	30	40	50
Danesino	1st study	2.500	3.850	4.350	4.100	4.050
	2nd study	2.075	2.325	2.225	2.375	2.500
	3rd study	3.600	3.888	3.288	3.425	3.288
Gatti	1st study	1.950	2.400	2.550	2.575	2.900
	2nd study	1.450	1.975	2.075	2.575	2.150

The main conclusion we may draw from the values given above is, as follows: (1) the difference-threshold for the perception of length in tactual space does not follow Weber's law; *i.e.* while the absolute limen does not increase according to the length of the stimuli, it approaches a constant value, at least in Danesino's second and third researches; (2) Gatti's results appear to vary by small regular steps according to the length of the stimuli.

Referring to the experiments themselves, whereas Danesino stated that by the second and third researches he was able to perceive tactual stimuli without associated visual images, on the other hand, I was never able to get rid of them. We may also state that the difference-threshold (y) is related to several independent variables: viz., x length of stimuli, s subject, b processes other than touch, t pure sensations of touch, c area of the skin stimulated. The function may be written as follows: $y = f(x, s, b, t, c)$.

Since y can never be equal to zero, we have to admit a factor (a), which works as a personal constant. On the other hand, as the subject remains the same, we may figure the relation existing between y

[6] Alessandro Gatti, Le rappresentazioni spaziali nel campo tattile puro, *ibid.*, 14, 1936, 69-84.

and the other variables as follows: $y=(a/c)x^{b/t}$; that is to say, the difference-limen varies according to a certain power of the stimulus. Taking into account the fact that sensations of touch remain the same throughout all the experiments, we have to consider t as a pure figure. Since my own results (see Table II) appear to vary according to a cubic parabola, the relation written above may be stated as follows: $y=(a/c)x^{b/3}$. Finally, as the area of the skin (c) has the same sensorial qualities throughout the series of stimuli, the ratio $a:x$ may be considered as a constant value and the relation will be as follows: $y=ax^{b/3}$. The difference-limen, in other words, varies according to the cubic root of a certain power of the stimulus. Making the value of $t=3$, as b means the variable related to the influence of visual images, we can say that it varies from the minimum value of zero to the maximum of 3 according to the subjects. We have then the following relations:

$$\left(\lim_{b=0}\right) y=\left(\lim_{b=0}\right) a\,x^{b/3}=a$$

$$\left(\lim_{b=3}\right) y=\left(\lim_{b=3}\right) a\,x^{b/3}=a\,x$$

The first relation shows the course of Danesino's results (see Table II). The second relation refers to the case when the visual qualities of space over-rule all other sensations or associated images. This relation expresses the course of the difference-threshold in vision as shown by Kiesow's results.

We have also a relation which varies according to the rôle played by the different components of psychological space as a unity. As a matter of fact, the experimental data confirm this hypothesis.

The values of a and $b/3$ drawn from my own experimental determinations are given in Table III. If the theory be good, these two factors should be constant throughout the series of the stimuli employed. Further, the average of a and $b/3$ allows the calculation of the limen, as it should be, for each stimulus. A resumé of these results are given in Table III, Δr meaning the limen.

The data of Table III show that the average difference between calculated and experimental values amounts to about 5%. This fact leaves no doubt about the course of variation of Δr.

As to Danesino's results, it may be remarked that he had practically no visual images. To express this fact we have to make $b = 0$, viz. $y = a$. As a matter of fact, Danesino's data approach a constant

throughout the stimuli. This is a further proof that the difference-limen is dependent upon the rôle played by the various processes which take part in the perception of space.

TABLE III

A Resumé of the Results of Gatti's Studies

Study	Stimulus lengths	a	$b/3$	Δr Exp.	Δr Cal.	Diff.
First	10	0.905109	0.371988	1.950	1.732	+0.218
	20	0.884167	0.278367	2.400	2.162	+0.238
	30	0.820666	0.330707	2.550	2.462	+0.088
	40	0.752934	0.307631	2.575	2.700	−0.125
	50	0.787181	0.320400	2.900	2.900	0.000
	Geo. mean	0.828031	0.320379	Arith. mean		0.134
Second	10	0.673030	0.329880	1.450	1.462	−0.012
	20	0.727596	0.356701	1.975	1.842	+0.133
	30	0.667797	0.328700	2.075	2.109	−0.034
	40	0.752935	0.361589	2.575	2.321	+0.254
	50	0.583600	0.294855	2.150	2.501	−0.351
	Geo. mean	0.678404	0.333470	Arith. mean		0.157

To check this fact we may also refer to Oberto's research on the difference-threshold in the alignment of three punctiform stimuli pressed upon the skin.[7] Oberto's apparatus was designed by myself in such a way as to allow a sure reading of the minimum angle perceived by the subject in the alignment of the stimuli. Oberto used the same psychophysical method as Danesino did for his research. He employed however, 7 stimuli, the points stimulating the skin being placed at the distances of 10, 20, 30, 40, 50, 60, and 70 mm. The area of the skin stimulated was the left inner fore-arm.

While the minimum angle perceived approaches a constant value in vision, the results obtained by Oberto show that the same angle varies in touch by decreasing steps. The variation is such as to keep almost constant the linear distance to which one of the stimuli has to be moved in order to see an angle between them. The relation for vision— $\epsilon/d = tg\alpha = k$— does not, therefore, apply to touch.

We may assume, however, that in this case too the limen is dependent upon the same causes as for the evaluation of length. We can also write: $y = ax^{b/3}$, where y means the displacement in mm. of one point in regard to the others, a is a personal constant, b the influence of processes other than touch (visual images). If b has the maximum value of 3 as in vision, we have: $y = ax$. In this case a means the tangent of the minimum angle perceived, which keeps the same throughout the

[7] Stefano Oberto, La soglia di rettilineità nel campo tattile puro, *ibid.*, 14, 1936, 85-95.

stimuli, as happens in visual experiments. As to Oberto's research on the skin we can apply the above written relation in the same way as we did in regard to the difference-limen of length.

In the following table (Table IV) there are given the values of a and $b/3$, calculated from the experimental data, together with the data themselves and their corresponding values, drawn by calculation from the average of a and b. The data refer to the first 5 stimuli employed by Oberto.

TABLE IV

RESULTS OF OBERTO'S STUDY

			Angular Threshold		
Stimuli	a	$b/3$	Exper.	Calcul.	Diff.
10	1.008300	0.383776	12° 15′ 22″	10° 59′ 38″	+1° 15′ 44″
20	0.879385	0.326438	6° 48′ 22″	6° 59′ 12″	−0° 10′ 50″
30	0.801450	0.300657	4° 45′ 22″	5° 20′ 49″	−0° 35′ 27″
40	0.910232	0.337079	4° 27′ 00″	4° 25′ 14″	+0° 1′ 46″
50	0.901457	0.334390	3° 48′ 00″	3° 48′ 47″	−0° 00′ 47″
Geo. mean	0.897748	0.335264		Arith. mean	0° 24′ 55″

	Linear Threshold		
Stimuli	Exper.	Calcul.	Diff.
10	2.17233	1.94274	+0.13009
20	2.38702	2.45098	−0.06396
30	2.49606	2.80786	−0.31180
40	3.11295	3.09217	+0.02078
50	3.32009	3.33238	−0.01139
	Arith. mean	0.10760	

No further data are necessary in order to show that the difference-threshold of rectilinearity follows the same law as the preceding limen for the perception of length. The average difference between experimental and calculated values is so small (0.1 mm. and 47″) as to justify our thesis.

The first conclusion we may draw, from what has been said above, is as follows. Whereas stimuli may vary by infinitesimal degrees, our perception of space varies only by limited ones. Weber's law is a particular case of this more general principle, which, in my opinion, is a proof that our perceptive world is based on the line of the greatest psychological simplicity.

I think we may further assume that the above mentioned experiments supply another proof in support of the hypothesis of the psychological unity of space.

CONCEPTIONS OF MENTAL FATIGUE

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We might be expected to know what we mean by mental fatigue, even though we find difficulty in verbally defining it. For 'fatigue,' like 'force' and 'activity' to which it relates, was originally descriptive of *subjective* human experience. Just as we are conscious of the forces which we exert and of our self-activity, so we are conscious of feelings of fatigue. But the use of all three terms has long since been widely extended in an *objective* sense—to external mechanical 'forces,' to the 'activity' of lifeless objects, and to the 'fatigue' even of metallic substances.

By the physiologist 'fatigue' is commonly regarded as the expression of a decline in the activity of a living tissue or organ caused by its previous activity.[1] Such an objective definition, however, presents several practical difficulties: in the first place, when we consider the activity not of an isolated organ but of the intact organism; and secondly, when we are considering not bodily, but mental, activity. For in the intact organism, as is well known, the signs of muscular fatigue may be manifest not only in active muscles but also in others far distant and resting, if the products of the activity of the former—or indeed other toxins—are conveyed to the latter by the circulation. There is no doubt that the activity of the central nervous system is similarly impeded by toxic substances and by psychological factors of distant origin.

Moreover, in the intact organism muscular fatigue may result in temporarily *increased*, not diminished, motor activity. It seems as if a normally unavailable store of muscular 'energy' is always held in reserve through the control exercised by inhibitory nervous processes, but that this control is lessened or abolished in certain circumstances, *e.g.* under emotional excitement, through the influence of alcohol, or owing to certain other conditions one of which is 'fatigue.' Conse-

[1] E. S. Adrian, *The Basis of Sensation*, 1928, 67.

quently the 'fatigue' due to muscular activity may be initially accompanied by a rise, not by a decline, of muscular activity. In similar circumstances, the output of *mental* work may be temporarily increased in quantity or quality. This may occur, for example, in the initial 'fatigue' that results from severe deprivation of sleep,[2] or from the incentive of an 'end-spurt' at the close of a long spell of ultimately diminishing mental activity when once the worker becomes aware of the approaching end of his task. But the recognition of controlled 'reserves' of 'mental energy' does not necessarily imply the existence of a single common store of it which may be switched on so as to excite, or drained away so as to inhibit, any kind of mental activity. Neither our scant physiological knowledge of inhibition nor our present psychological conceptions of repression give adequate support to such a view.

At the outset, then, we have to distinguish between the efficacy of controlling, inhibiting activities and the efficacy of activities which they control or inhibit. This distinction is particularly important in regard to the mind. At the one extreme stands the mental work which involves constant, conscious, controlling direction; at the other extreme stands the mental work which is of a routine, almost automatic, character. That is to say, we distinguish between the processes of purposeful self-activity and the more 'mechancial' processes employing already acquired associations or integrations. Each, of course, possesses certain characteristics of the other: even at the lowest levels mechanisms of inhibition and coördination are to be found. Consequently no hard-and-fast line can be drawn between 'fatigue' of the more 'directive' and 'fatigue' of the more 'mechanical' mental processes. But we may expect 'fatigue' of the former to express itself rather in disorder of activity, and 'fatigue' of the latter to express itself rather in stoppage of activity. And we shall need to consider the 'fatigue' not only of these two kinds of mental activity—the one relating to attention, apprehension, intelligence and creative activity, and the other relating to the mere reproduction of ideas and the repeated performance of habitual acts; but also the mental 'fatigue' of more deeply unconscious acts and the 'fatigue' of purely sensory and affective processes.

Feelings of Fatigue and of Boredom

We have observed that *objective* behaviour, as evinced in diminishing quantity or quality of output, is an uncertain criterion of mental

[2] Cf. May Smith, A contribution to the study of fatigue, *Brit. J. Psychol.*, 8, 1916, 327-350.

fatigue. Equally unsatisfactory is the *subjective* experience of mental fatigue: we may show outward signs of mental deterioriation due to 'fatigue' without feeling fatigued, or, on the other hand, we may feel fatigued in the absence of objective evidence of such deterioriation due to 'fatigue.'[3]

Moreover, we have to bear in mind that prolonged mental activity may give use not only to feelings of mental *fatigue* but also to feelings of *boredom*. Here too the quantity and quality of output will be impaired; yet they are associated with the subjective experience of boredom, not with that of fatigue. We recognize at once the fundamental difference between these two experiences. And the remedies for them are essentially different—true fatigue requiring rest from work, mere boredom requiring only a change of work, for their respective alleviation.

When we attend to a prescribed mental task, we inhibit other fields of attention, and their contents, which are incompatible with the execution of that task. At first such inhibition may be involuntary, because we are immediately, intrinsically, interested in our work: the particular and requisite 'posture' of our attention is established and maintained through sheer, immediate interest. But, sooner or later, what is thus spontaneously inhibited refuses any longer to remain inhibited. As immediate interest increasingly wanes, indirect and remoter interests must be volitionally summoned; and volition has to be invoked more and more to exercise that repression of untoward ideas and irrelevant fields of attention which was at first performed quite involuntarily. As these various interests wane and as no others are available to take their place, boredom appears on the scene. Boredom is thus the outcome of a failure to find interests which can maintain spontaneous or voluntary attention. When in due course voluntary attention first becomes impaired, the 'fatigue' of attention which now enters takes on a progressively higher and more profound character—a 'fatigue' of general self-activity instead of as at first, a failure (or 'fatigue,' if we may so term it) of specific interests. For the still further prolongation of mental activity results ultimately in a release of unconscious 'complexes' of emotional colouring hitherto kept under control; in irritation, worry, anxiety or obsessions; and in an inability to recall wanted memories, to apprehend and consider all relevant issues and relations, to draw inferences and frame decisions, to see any problem as a whole and to effect its solution—in a general impotence

[3] Cf. B. Muscio, Feeling-tone in industry, *Brit. J. Psychol.*, 12, 1921, 150-162.

to concentrate attention and to act purposefully, intelligently and creatively.

Fatigue and Affective Processes

The close connexion between emotional disorder and the psychoneuroses and psychasthenia is universally admitted; so too is the narrow line between physiological and pathological conditions. We may well wonder, therefore, whether affective disturbances (and their immediate causes) are not also concerned with the normal impairment of prolonged mental activity. Just as certain unconscious processes, connected with interest and other felt incentives, spontaneously effect, or at least maintain and increase, the direction, coördination and repression required for the proper execution of a given mental task; just as certain unconscious processes, associated with the feeling of boredom, may be supposed to weaken such spontaneous direction, coördination and repression, and thus to safeguard the organism from severe local, specific, mental fatigue—may not other unconscious processes, connected with feelings of fatigue, be responsible, in part at least, for the lack of voluntary direction, coördination and repression so characteristic of 'fatigued' higher and more general mental processes? In other words, may not our feelings of mental fatigue, and the unconscious processes relevant to these feelings, be the *cause* rather than the *consequence* of the impairment in mental output following prolonged mental activity? May not our feelings of fatigue, muscular indeed as well as bodily, be due, in part ar least, to the influence of endocrine secretions probably on the hypothalamus and basal ganglia and to their well-known affective and other mental causes, concomitants, or consequences? How else can we explain the feelings of severe bodily fatigue that may result from extreme grief or from intellectual strain, or on the other hand, the proneness to take bodily exercise that sometimes results from extreme mental efforts? Why otherwise should serious mental fatigue be felt, and impairment in output occur, when creative work is being consciously performed but hardly at all when it is performed unconsciously in a dream-like 'dissociated' state? Surely the affective processes, conscious and unconscious, when not dissociated from the 'personality,' must be a potent factor in determining the signs and symptoms of fatigue.

We may therefore well wonder whether the felt fatigue consequent on strenuous mental activity reflects at all stages a really fatigued state of that activity, or whether we should not rather envisage it as an

inhibitory protection of such activity from serious fatigue. That is to say, we may ask ourselves whether we have always to deal with a direct *fatigue* of inhibitions and coördinations or, in certain stages at least, rather with a possible *inhibition* of inhibitions and coördinations through affective (conscious or unconscious) processes?

In favour of the notion of *fatigue*, it may be argued that a spinal reflex that ceases under *inhibition* shows none of the characteristic changes which occur in the spinal reflex that suffers from *fatigue*. As the flexion reflex tires, it becomes less steady and more tremulous; brief periods of intermission occur, and even its replacement by the hitherto successfully inhibited, antagonistic act of extension. So, too, as the scratch reflex tires, its beats become slower and more irregular in amplitude, tending to be arranged in groups and spasmodically to disappear and to re-appear. Now these changes are also fairly and usually characteristic of the impairment of directive mental activity. None of them, however, occurs in *inhibition* of the spinal reflex:[4] the spinal reflex that ceases under inhibition fades away without them. But this argument may be met by the retort that the possible inhibition of inhibitory and coördinating activity may be quite different from that of excitatory activity.

Cerebral Fatigue

On the other hand, in favour of the view that higher mental 'fatigue,' is 'apparent' rather than 'real,' it has been urged that the cerebral hemispheres are so abundantly supplied with blood and that their consumption of physical energy is so minute that the existence of true cerebral fatigue seems very questionable. To this it may be replied that the electrical pulsations, indications of cerebral cortical activity, vary even under deep anaesthesia between states of intense activity and complete rest, and that neither state can continue for long.[5] A further difficulty arises from the fact that mental breakdown may sometimes suddenly result from the strain of prolonged mental work without the conscious fore-warning of unfavorable affective processes, and that it may sometimes be dissipated merely by complete rest, massage and appropriate feeding, without recourse to any of the affectively directed forms of psychotherapy. But such breakdown may be directly ascribed to toxic conditions, perhaps again of endocrine origin, due to previous strain, much as may be the well-known 'con-

[4] Cf. C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906, 214-221.
[5] Cf. E. D. Adrian, *Factors Determining Human Behavior*, 1937, 6.

fusional' state associated with emotional overstrain and removed merely by rest and suitable feeding.

We have also to bear in mind the recognition which is gradually gaining that we have no right to speak of localised 'centres' of consciousness. There is a large body of experimental work that points to a wide 'equi-potentiality' throughout the cerebral hemispheres.[6] When, through disease of, or injury to, one limited cortical area, one particular kind of conscious activity is abolished, we are not justified in assuming, as has been so commonly assumed, that that kind of consciousness necessarily has its *seat* in that area. The more correct interpretation is surely that that relatively narrow area is normally essential for that particular kind of consciousness—that nervous impulses must normally pass through that area in order that that kind of consciousness may arise. Moreover, consciousness implies what we vaguely term 'self-activity;' and self-activity involves the mass-activity of wide areas of the cerebral cortex, if not also of other regions.

At first sight, the effects of prolonged attention might be thought to be strictly limited to its particular field, and only later to extend so as to affect attention in general. This may hold true for the more local conditions of 'boredom,' but is hardly likely when boredom is replaced by 'fatigue.' In this connexion it might also be supposed that there are relatively or extremely 'special' abilities, *e.g.* mechanical ability or manual ability,[7] which may be impaired by use without involving wide-spread impairment; whereas there are more 'general' abilities, *e.g.* general intelligence, the excessive use of which may lead to general impairment of higher mental activity. But in fact no single 'ability' is ever simple or employed alone; no higher mental operation is repeatedly performed in exactly the same way; and none voluntarily without concomitant attention.

Even within any one cerebral area it would seem likely that so many alternative neural paths are possible, that as some of them hitherto used become liable to fatigue, others may with virtually equal efficiency take their place. The almost indefatigable 'perseveration'—or persistence—of fixed ideas and of obsessions in psycho-pathological states would thus be explained. We know too that the impotence that marks the end of an ergogram can be readily and immediately dispelled by slightly reducing the weight that has been lifted. The exercised muscle has been sending inhibitory impulses to the central nervous

[6] Cf. K. S. Lashley, *Brain Mechanisms and Intelligence*, 1929, 186.
[7] Cf. J. H. Cox, *Manual Skill*, 1936.

system which finally result in an apparent 'fatigue' of the neuro-muscular system—but merely to that particular strain or 'posture:' the muscle will at once respond again to an altered 'posture.' a somewhat similar conception is applicable at least to the *initial* impairment consequent on a prolonged 'posture' of attention.

Habitual Acts and Fatigue

So far we have been considering the mental activity that involves conscious direction. Let us turn now to a consideration of such almost automatic mental work as is involved in adding successive pairs of single numbers (*e.g.* 2 +3), or of erasing one or more prescribed letters in lines of printed text. When such responses suffer as they do, in diminished quantity or quality or in increased variability of output, through repeated elicitation, surely the result is due far more to defects of attention than to fatigue in the habitual processes themselves.[8] We reach a state when we can no longer preserve the requisite 'posture' towards the task; we fail any longer to apprehend readily the *meaning* of the situation presented. That process of apprehension need not be consciously active, but it does depend on a certain posture of attention which becomes impaired, if too long maintained. If we could but apprehend, even unconsciously, the presentation 7 + 9, would the correct response be really delayed, or replaced by an erratic response—through sheer fatigue of mechanically acquired meaningless association? If we realised, even, unconsciously, the prescribed letter in a given word of the text and the appropriate response, should we—apart from muscular fatigue—take appreciably longer to cross it out? A little, perhaps at first, and ultimately (like reflexes) perhaps enormously if higher fatigue were not earlier and more important; but that is surely not the main cause of fatigue in such mental work.

Turning to another kind of thoroughly practised activity, the execution of a skilled manual operation, 'fatigue' is apt to manifest itself in the resuscitation of older, unskilled activities which the later acquisition of skill has hitherto inhibited. Former bad habits, previously discarded through inhibition, but not (be it observed) obliterated, come again to the surface. Once more we meet with 'fatigue' of an activity manifesting itself as a 'fatigue' of the inhibitory processes involved in that activity.

[8] Cf. T. F. Weiskotten and J. E. Fergusson, A further study of the effects of loss of sleep, *J. Exp. Psychol.*, 13, 1930, 247-266.

Fatigue and Adaptation

There can be little doubt that, viewing broadly the activity manifested by living substance, it falls into at least two main divisions and that these have two different physiological bases. The one kind of activity is relatively mild, 'discriminative' and continuous; the other relatively intense, 'mechanical' and rhythmical or spasmodic. The physiological basis of the latter is often the consumption of a particular material secreted by the living cell expressly for its specific activity; that of the former seems especially dependent on coördination with other cells of the organism. We believe, for example, that the resting striated muscle-fibre stores up a glycogen-like substance having some relation (at present uncertain) to the moment when it is stimulated to contract. This is exploded, as it were, in (or through) an 'all or none' reaction; and rest is again needed for the muscle-fiber to manufacture fresh substance before it is prepared to contract again. The heat and cold spots of the skin display similar intense and spasmodic activity, exhibiting also the 'all or none' reaction and needing rest after each response to stimulation.

But the 'tone' of skeletal muscle is relatively indefatigable: like posture, it appears to be dependent essentially on an appropriate reciprocal and unchanging balance between the generally mild, excitatory and inhibitory, nervous impulses that continually play on it. In the field of mild sensation, too, we can endure the ceaseless stimulation of diffuse daylight throughout a Norwegian summer or of a moderately loud sound throughout the day, without evidence of sensory fatigue. Our *attention* to the light or sound may cease; but at any moment—unless the stimulus be very faint indeed—we can resume attention to it. In the case of a weak, protracted, colour or temperature stimulus, our conscious awareness of it also disappears. But this again cannot be ascribed to direct fatigue of the sense organ: it seems due rather to a balance between two antagonistic sensory processes, *e.g.* between blue and yellow, or between warm and cool, the excitation of one of which involves initially the inhibition, succeeded by an increasing excitation, of the other—until a stage of equilibrium is reached and the colour or temperature sensation ceases. This condition of equilibrium, known as sensory adaptation, is, however, unstable. For if the now no longer sensed stimulus be removed, the antagonistic, originally inhibited, process continues to be excited and, by such 'negative induction' the contrasting sensation is developed, until a state of equilibrium, now at a stable potential, is again reached. An

apparently similar state of adaptation and of successive contrast occurs in prolonged states of pleasure or displeasure.

The Fatigue of Nerve Fibres

But it is difficult to see what lessons these phenomena offer us in regard to the higher mental fatigue of directive and creative ability: we know so little of cerebral activity and of its relation to mental activity. We know that medullated nerve fibres, like striated muscle fibres and warm and cold spots, exhibit an 'all or none' reaction, and, like them, vary among one another in the minimal strength of stimulus required for their response. We have for long recognized that each nervous impulse is followed by a brief 'refractory' period: and certain experimental psychologists have fancifully likened to these the occurrence of what have been termed 'prolonged' or 'cumulative' refractory periods in repetitive mental tasks, varying in frequency and length with the individual, with the task and with increasing mental fatigue.[9]

We no longer believe, as we once belived, that nerve fibres are indefatigable. The repeated conduction of impulses along them is accompanied not only by prolongation of their refractory period, but also by diminution in their consumption of oxygen, in their production of heat, in their speed of conduction of the nervous impulse, and in their irritability. These changes, however, progress only for a short time: upon continued stimulation, a stage of equilibrium is reached between further changes and recovery—a condition of raised threshold and lowered speed of conduction—which has received the name of 'accommodation.'[10]

We have now evidence that in the sympathetic ganglia, at least, the dendritic processes of the neurone depend, for the conduction of their excitatory nervous impulses to other neurones, on the secretion and consumption of certain chemical bodies (*e.g.* acetylcholine) at the synapses, which have the power of transmitting nervous impulses. We have also reason to believe that, owing to the exhaustion of the acetycholine which they release, the terminations of nerve fibres in the striated muscle fibres, known as end-plates, can block the passage to the muscle fibres of a frequently repeated or continuous stimulus applied to the nerve fibres, long before the muscle fibres themselves are

[9] Cf. A. G. Bills, Blocking: A new principle of mental fatigue, this Journal, 43, 1931, 230-245; Fatigue, oscillation, and blocks, *J. Exper. Psychol.*, 18, 1935, 562-573; and C. W. Telford, The refractory phase of voluntary and associative responses, *ibid.*, 14, 1931, 1-36.

[10] Cf. R. W. Gerard and W. H. Marshall, Nerve conduction, velocity, and equilibration, *Amer. J. Physiol.*, 14, 1933, 375-383.

unable to respond to the same stimulus when directly applied to them. All this knowledge, however, gives us very little light on the subject of higher mental fatigue, such as affects purposeful and creative activity rather than the comparatively mechanical activity of spinal reflexes and other innate or acquired reactions.

Fatigue and the Nerve Cell

But it is surely not an improbable assumption that the nerve-cell is something more than the mere transmitter (or inhibitor) of the nervous impulses which it receives, and that the fatigue of mental activity is consequently something more than the relations between secreted chemical substances, like acetylcholine, and synaptic resistance. The living nucleated cell-body of the neurone is the very life of the cell. The rhythmic movements of breathing, running and walking are independent of external stimuli for their occurrence: they are due to automatic cycles of activity in the nerve cells themselves. Here surely is the source of those diurnal variations in mental capacity, those organic rhythms of mental efficiency, which are to be found in an individual whether he works or rests. [11] Here, above all, we should most confidently look for the chief distinction between creatively living and completely lifeless activity, and not only at the synapses for the exquisite sensitivity of the nervous system to chemical substances (caffeine, etc., and toxins) diffused through and carried by the circulatory system. But it is here that we are confronted with the most profound ignorance and with the greatest difficulties in research.

Even the physiological effects of continuous or frequently repeated stimulation of a motor area in the cerebral cortex afford little more information in regard to mental fatigue than that which we have already gleaned. They consist first—and interestingly enough in relation to the opening paragraphs of this article—to an *increase* in the response evoked; next—as we might expect from our knowledge of spinal reflexes—to an increase in the tendency to appearance of the antagonistic, originally inhibited, movement; and finally to a spread of the response to those of neighbouring motor areas, apparently a kind of epileptiform reaction characterized by a lack of order and coördination and by a wasteful dissipation of 'energy'—conditions not unlike those of 'nervous break-down' in the intact human organism. Ultimately, if long enough continued, cortical stimulation leads

[11] Cf. B. Muscio, Fluctuations in mental efficiency, *Brit. J. Psychol.*, 10, 1920, 327-344.

to failure of responses, perhaps partly through the local anaemia thus produced.

SUMMARY

Enough has been said to indicate not only that there are at present divers conceptions of mental 'fatigue' and of protections against it, but also that—whether these are adequate or not—mental 'fatigue' must always demand divers conceptions for its complete description. Prolonged exercise may affect both the controlling and directing activities and the activities which they control or direct. The feelings of fatigue and other affective processes appear to exercise a protective influence in preventing excessive use of volitional activity. And no doubt the presence, neutralisation or exhaustion of chemical substances, favourable or unfavourable to mental activity, and changes in electrical potential, are ultimately responsible not only for reducing excitation but also for inducing inhibition. As Pavlov's work on conditional reflexes indicates, "the cortical elements enter sooner or later into an inhibitory state. . . . The progressively developing inhibition . . . assumes the rôle of a protector of the cortical elements, preventing an excessive fatigue or dangerous functional destruction of this highly sensitive structure."[12]

We can distinguish objectively and symptomatically between the mental fatigue which is dissipated by ordinary rest or sleep and the more serious psychoneurotic or psychasthenic results of overstrain which usually demand psychotherapeutic measures for their cure. But there seems to be no definite line psychologically between such physiological and pathological fatigue: similar psychological processes in different degrees are involved in each. Although these, in fatigue as in practice, are invaluable sources of illumination, the future hope of 'explaining' mental fatigue lies in its description in physiological terms—in its attribution to processes that involve known material activity. We know already a good deal about muscular activity; and we know something of the excitatory activity of the nerve fibre. But we know relatively little of the physiological basis of central inhibition, which is so essential for coördination, higher control and purposive mental activity. And we know absolutely nothing of the physiological basis of creative mental activity, conscious or unconscious. At present we can only describe mental fatigue in terms of subjective feeling and of objective behaviour. But up to now, at least, these have afforded little more than various possible conceptions of its nature.

[12] I. P. Pavlov, *Conditioned Reflexes* (Eng. trans.), 1927, 250.

STAND UND LAGE DER ANGEWANDTEN PSYCHOLOGIE IN DEUTSCHLAND

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Von den mannigfaltigen Gebieten der angewandten oder praktischen Psychologie in Deutschland wollen wir insbesondere die industrielle und Wirtschafts-Psychologie behandeln, da die Entwicklung gerade dieses Fachgebietes in Deutschland besonders typisch und ergebnisreich war.

Der Mensch steht im Mittelpunkt aller Studien in der industriellen und Wirtschafts-Psychologie, die sich in zwei Hauptrichtungen gliedert: die praktische Psychologie der Produktion oder Fertigung sowie die praktische Psychologie des Konsums. Ihre Arbeitsgebiete sind: (1) Eignungsfeststellung; (2) Anlernung und Schulung; (3) Arbeitstechnik als Bestgestaltung der Arbeitsverfahren; und (4) Markt-, Verkaufs-, und Werbungspsychologie.

Eignungswesen und Eignungstechnik als Glied der Personalverwaltung

Die Entwicklung der Eignungsfeststellung auf psychologischer Grundlage war in Deutschland rasch und lebhaft, besonders nachdem während des Weltkrieges im Jahre 1915 psychotechnische Eignungsprüfungen bei den deutschen Kraftfahrtruppen erfolgreich eingeführt wurden.[1] Die deutsche Reichsbahn und zahlreiche andere Verkehrs- sowie sonstige Betriebe bedienten sich bald der Eignungsfeststellung. Geltende Anschauung ist heute, dass ein Verkehrsbetrieb im Rahmen der Sorgfaltspflicht seiner Personalverwaltung, Eignungsfeststellungen auf psychotechnischer Grundlage mit einsetzen muss, um seinen sozialen Verpflichtungen als Arbeitgeber zu genügen. Das höchste deutsche Gericht entschied, dass die Psychotechnik zuverlässigere Mittel an die Hand gibt, "die Reaktionsfähigkeit eines Menschen in Hinsicht auf

[1] Walther Moede, *Lehrbuch der Psychotechnik*, I, 1930, 429 ff.

zweckmässiges und schnelles Handeln festzustellen, als es eine mehrstündige, wenn auch noch so sorgfältige Beobachtung des zu Beurteilenden im Sitzungssaale vermag."

Neben den Verkehrsbetrieben, die bahnbrechend wirkten, waren es insbesondere die psychotechnischen *Lehrlings*-prüfungen der Grossindustrie, die dem Gedanken einer wissenschaftlichen Eignungsfeststellung in Deutschland bald Eingang verschafften.

Die Entwicklung führte zu 3 verschiedenen Formen von psychotechnischen Eignungsprüfstellen. Wir finden in Deutschland: (1) wissenschaftliche und Forschungsstellen an den Hochschulen; (2) Betriebsprüfstellen; und (3) staatliche soziale Einrichtungen der Arbeitsämter.

FIG. 1. AUFBAU DER EIGNUNGSTECHNIK BEI DER DEUTSCHEN REICHSBAHN

Die Hochschulen als neutrale Stellen pflegen sowohl Forschung als auch praktische Begutachtung durch Ausarbeitung, Überwachung und Fortbildung von Eignungsfeststellungen aller Art sowie Schulung des wissenschaftlichen Prüfpersonals. Das Institut für Industrielle Psychotechnik und Arbeitstechnik der Technischen Hochschule zu Berlin, das zunächst 1918 eine Industriegründung war, wurde bald verstaatlicht und konnte eine Vielzahl von Ingenieuren in diesen neuen Zweig psychologisch ausgerichteter Betriebslehre einführen, die sich auf den Faktor Mensch im Betriebe bezieht und die an den Technischen Hochschulen der technischen und wirtschaftlichen Betriebslehre ergänzend zur Seite tritt.

Als Beispiele für Betriebsprüfstellen erwähnen wir die Einrichtungen der Deutschen Reichsbahn, die Fig. 1 im Überblick zeigt.[2] Jährlich gehen etwa 24 000 Mann durch die bei den einzelnen Direktionen vorhandenen Prüfstellen bei einem Personal von etwa 600 000 Mann.

[2] Moede, Psychotechnische Eignungsprüfungen bei den Eisenbahnen Europas, *Industr. Psychotech.*, 14, 1937, 33-41.

Bewegliche Prüfeinrichtungen, psychotechnische Prüfwagen, sind neben den ortsfesten Untersuchungsstellen vorhanden. Aufgabe derartiger Betriebsprüfstellen ist es, der raschen und guten Ersatzgestellung eines Unternehmens zu dienen. Bei der Deutschen Reichsbahn ist die psychotechnische Untersuchung obligatorisch. Berufseignungsprüfungen, insbesondere für Jugendliche, sind auch in die geltenden Tarifverträge aufgenommen.

Den Belangen der Wirtschaft eines kleinen oder grösseren Bezirkes dienen die öffentlich-sozialen psychotechnischen Ämter im Rahmen der Reichsanstalt für Arbeitsvermittlung, die der besten Einsatzmöglichkeit eines Jugendlichen oder eines Stellungslosen zu dienen berufen sind.

Kennzeichnungswerte der Persönlichkeit. Die Kennzeichnungswerte der Persönlichkeit, die als Grundlage der psychologischen Praxis Verwendung finden, sind in Tabelle I zusammengestellt.

TABELLE I

KENNZEICHNUNGSWERTE DER PERSÖNLICHKEIT

1. Erbgang und Erbwerte	7. Schriftbewertung	
2. Lebens- und Arbeitsschicksal	8. Leistungsproben mit	
3. Aussprache und Wechselrede Bericht—Vernehmung	fachlicher / charakterologischer	Wertungsmöglichkeit
4. Ausdrucksverhalten der Gesamtpersönlichkeit	9. Verhaltensproben	
5. Physiognomie und Mimik	fachlicher / charakterologischer	Ausrichtung
6. Sprechanalyse	10. Kombinierte Kennwerte	

So mannigfaltig der Widerstreit der Richtungen der theoretischen Psychologie ist, so erfreulich ist ihr gemeinsamer harmonischer Einsatz für die Zwecke der Persönlichkeitskennzeichnung und -wertung, die sich somit auf eine *synthetische Psychologie* stützt als Inbegriff aller praktisch verwendbaren Gliederungen der theoretischen Psychologie überhaupt. Aufgabe der Eignungsbegutachtung ist die Ermittlung berufswichtiger Persönlichkeitswerte zum Zwecke der beruflichen Bestverwendung, die unter berufstypischen Bedingungen abgeleitet werden. Das Ziel der Eignungsprüfung ist ein praktisches, nämlich die Voraussage zweckmässiger Berufseingliederung des Arbeitsuchenden nach Massgabe seiner Veranlagung. Die Mittel der Diagnose werden den zahlreichen Richtungen der theoretischen Psychologie entnommen, die die Grundlage für diese neue synthetische Psychologie bilden.

Das biographische Prinzip wird wirksam bei Bewertung des Erbganges und der Erbwerte sowie des Lebens- und Arbeitsschicksals, wobei die medizinische Konstitutionslehre nicht unberücksichtigt bleibt. Die geisteswissenschaftliche Psychologie, die mit Einfühlung

und Verstehen arbeitet, wird fruchtbar bei Aussprache und Wechselrede mit dem Prüfling, um Einblick in die Grundlinien der Persönlichkeit zu erhalten sowie ihre Haltung insbesondere auch zu Grundwerten der Arbeits-, Wirtschafts-, Lebens- und Staatsordnung. Planmässige Beobachtung und Darstellung gefundener Wesenszüge wenden das Rüstzeug der descriptiven oder beschreibenden Psychologie an, während die zergliedernde Betrachtung für Sicherung der Teilbestandteile im Ganzheitsbestande nicht entbehrt werden kann sowie als Voraussetzung einer erfolgreichen erklärenden und naturwissenschaftlichen Psychologie dient, die Funktionszusammenhänge aufdeckt und Leistung und Verhalten der Person im analytischen und synthetischen Verfahren kritisch überprüft, wobei sowohl Elementen- als auch Ganzheits- und Gestaltspsychologie zu ihrem Recht kommen.

Für die praktische Persönlichkeitskennzeichnung sind die verschiedenen Standpunkte der theoretischen Forschung Angriffsstellen und Mittel zur gesicherten Schilderung einer Persönlichkeitsveranlagung nach Leistungsfähigkeit und Haltung, wofür eine Vielzahl von Methoden in gegenseitiger Abstimmung zur Sicherung des Befundes sich als notwendig und nützlich erwiesen haben.

Das *Leistungsprinzip* hat sich als besonders erfolgreich bei der Begutachtung der Arbeitspersönlichkeit erwiesen. Teils begnügen wir uns mit einem Leistungs-*ergebnis* bei Lösung bestimmter Aufgaben und Ausführung von Proben der Sinnestüchtigkeit, des Aufmerksamkeits- oder Reaktionsverhaltens, teils untersuchen wir die Leistungs-*entwicklung* über einen längern Zeitraum, etwa 30–60 Minuten, unter berufstypischen Bedingungen, um Einarbeitung und Hereinwachsen in berufsnahe Aufgaben zu studieren, teils schildern wir den Leistungs-*typus*, zwecks Eingliederung einer bestimmten Leistung und Verhaltens in berufswichtige Grundtypen, teils wenden wir uns in der Hauptsache der Leistungs-*individualität* zu, die Schwerpunkt und Wert vorwiegend in der Eigenart ihrer strukturellen Gestaltung trägt.

Immer gibt es für alle Kennzeichnungsmittel der praktischen Psychologie einen untrüglichen und ausschliesslichen Prüfstein ihrer Bedeutung zur Wahrheitsermittlung: die Erfolgskontrolle der Entwicklung einer Person, die der Voraussage ihrer Leisting und ihres Verhaltens auf Grund der psychotechnischen Untersuchung gegenübergestellt werden kann. Aus solchen Erfolgskontrollen ergab sich der ungleiche Wirkungsgrad der verschiedenen methodischen Kennzeichnungsmittel für den Einzelfall, der von 0 bis annähernd 100 sich erstreckt.

Die deutsche Reichsbahn sowie die Industrieprüfstellen bauen alle ihre Prüfungen auf dem *Leistungsprinzip* auf. Die Auswertung erfolgt bei der Reichsbahn auf der Grundlage umfassender Erfolgskontrollen bei allen deutschen Stämmen, so dass die Eingliederung von Leistung und Verhalten auf der Grundlage der Werttabellen und Erfolgswerte des gesamten deutschen Gebietes erfolgt. Die Erfolgskontrollen und Bewährungsstatistiken zeigen, dass derartige Berufseignungsfestellungen auf Leistungsgrundlage recht hoch sind, liegen sie doch bei 90% und darüber. Als Beispiel der Erfolgskontrollen führen wir Tabelle II an.

TABELLE II

ERFOLGSKONTROLLE ASSISTENTENDIENST JAHRGANG 1932–1933

499 Bewerber	Eignungsgutachten
486 Übereinstimmung	1. Denkleistungen 2. prakt. Intelligenz
13 Versager, davon 4 Unterschlagungsfälle	3. Aufmerksamkeits-Reaktionsverhalten 4. Ruhe und Sicherheit bei der Arbeit
98% Übereinstimmung	5. Arbeitsdisposition, Arbeitssorgfalt u. —geschwindigkeit 6. Beobachtung 7. Auftreten

Auch die Kennzeichnung der Individualität eines Betriebsangehörigen kann nach dem Leistungsprinzip erfolgen. Für Berufsbewährung ist neben der Befähigung insbesondere auch die Arbeitseinstellung und Arbeitszuverlässigkeit wichtig, die an der Hand probeweis ausgeführter Leistungen sich offenbaren. Die Werturteile der Dienststellen über Arbeitszuverlässigkeit stimmen in hohem Masse mit den psychotechnischen Wertungen der Zuverlässigkeit der Arbeitspersönlichkeit überein.[8]

Dem Leistungsprinzip steht das Verhaltens- und Ausdrucksprinzip zur Seite. Das Ausdrucksverhalten wird bei bestimmten berufswichtigen Aeusserungen, und aus Leistung, Haltung, Sprache und Schrift erschlossen zur Kennzeichnung der allgemeinen und Arbeitspersönlichkeit, die damit nicht nur nach Leistungsfähigkeit, sondern auch nach allgemeiner Arbeits- und Lebenseinstellung beschrieben wird. Grundzüge der Arbeitspersönlichkeit allein freilich genügen mitunter nicht zur guten Berufsprognose ebensowenig wie auch die stärkste Häufung von fachlichen Befähigungsteilwerten, da schliesslich der Gebrauch von guten und schlechten Anlagen nicht nur von der fachlichen Befähigung allein, sondern auch von der Struktur und Haltung der Gesamt- und Arbeitspersönlichkeit mit abhängt. Die Ausdrucksdeutung für Zwecke der Berufsprognose ist nicht immer einfach, gilt

[8] I. Dilger, Eignung, Leistung und Charakter bei Wagenmeistern, *Industr. Psychotech.*, 11, 1934, 10-15.

es doch das Schein- vom Echtheitsverhalten zu trennen, sowie Zweck- und Gewohnheitsverhalten reinlich von den naturgegebenen Formen und Eigenschaften der allgemeinen und besonderen Haltung zu unterscheiden, um Fehldiagnosen zu vermeiden. Sicherungsmassnahmen der verschiedensten Art, beispielsweise als Simulations- und Dissimulationsprobe, Kontrollfeststellungen und Abstimmung der verschiedenen teils gleichartigen teils widersprechenden Teilbefunde sind unerlässlich.

Leistungs- und Verhaltungsproben in ihrer Gesamtheit, setzen uns in den Stand die Befähigung der Persönlichkeit in fachlicher und charakterlicher Beziehung für erfolgreiche Berufsprognose zu kennzeichnen.

Der Widerstreit zwischen den einzelnen theoretischen Richtungen, der sich auch gelegentlich in der Praxis äussert, ist unbegründet; nur der praktische Wert und der auf Grund der Erfolgskontrolle erwiesene Wahrheitswert entscheidet, um je nach der Lage des Einzelfalles die besten und zweckmässigsten methodischen Hilfsmittel einzusetzen, die richtige Urteile über einen Menschen gewährleisten. Die extremen sich bekämpfenden praktischen Richtungen liegen einmal in der ausschliesslich Mass- und Zahl verwendenden Methode der Persönlichkeitswertung, insbesondere engster fachlicher Art durch eine Vielzahl von Tests, wobei der Mensch als Träger dieser Eigenschaften und Eigenschaftsgruppen sowie Verhaltungsformen sowie Eigenart und Gepräge überhaupt nicht beachtet werden. Die andere polar entgegengesetzte Richtung verwirft mitunter alle positiven und methodischen Untersuchungsmittel, um lediglich durch Einfühlung, "Blick" sowie Deutung nach dem Ausdrucksprinzip den Kern der Gesamtpersönlichkeit zu erfassen, ohne Rücksicht auf das engere Begutachtungsziel, die Eignung für bestimmte Berufsarbeit. In Wahrheit sind alle Richtungen der psychologischen Betrachtungsweise für die Praxis in der Hand des Fachmannes bedeutungsvoll, besonders aber diejenigen Feststellungsmittel, die nach dem Leistungsprinzip ausgerichtet sind. Die Eignung eines Menschen besagt seine erfolgreiche Leistungsfähigkeit für bestimmte Aufgaben in Beruf und Leben, und sie kann am besten an der Hand geeichter und gesicherter Leistungsproben festgestellt werden, bei denen auch dem Leistungsverhalten auf Grund von Beobachtung unter berufstypischen Bedingungen, also bei Erfüllung wichtiger berufsnaher Aufgaben, voll Rechnung getragen wird.

Anlernung

Die Eignungsauslese setzt ein ausreichendes Angebot von Bewerbern voraus, die zwecks Arbeitszuteilung zur Verfügung stehen müssen. Ist

aber die Mehrzahl der arbeitsgeeigneten Personen nach Massgabe ihrer Einsatzfähigkeit in das Wirtschaftsleben eingegliedert oder wieder eingegliedert und besteht weiterhin Bedarf an brauchbaren Arbeitskräften, so hat die Betriebsführung Schulung und Umschulung auf psychologischer Grundlage zu organisieren, um die vorhandenen Lücken des Personalbestandes zu schliessen und die Leistung der Neulinge rasch und gut zu ertüchtigen.

Auf der Personalkarte des industriellen Gefolgsmannes, wie Fig. 2. zeigt, ist zunächst der Eignungsbefund niedergelegt, wobei den einzelnen Prüfgruppen I–IV die einzelnen berufswichtigen Seiten der Arbeitspersönlichkeit entsprechen. Darüber hinaus wird aber die

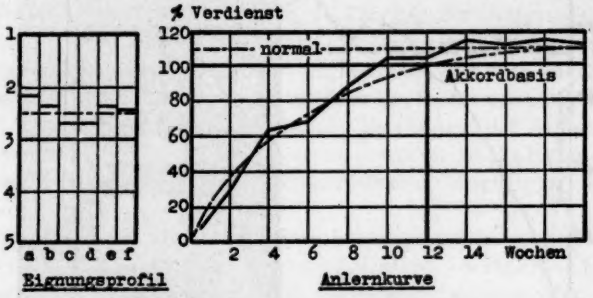

FIG. 2. EIGNUNGSPROFIL UND ANLERNKURVE

Übungs- und Anlernfähigkeit des Bewerbers bewertet, um seine voraussichtliche Einarbeitung und seine Entwicklungsfähigkeit an dem für ihn vorgesehenen Arbeitsplatz kennen zu lernen und zu kontrollieren. Die industrielle Psychologie Deutschlands hat für die hauptsächlichsten Typen der industriellen Arbeit Übungs- und Entwicklungskurven gewinnen können, so dass eine ungefähre Entwicklungsprognose ausgesprochen werden kann. Am Beispiel der Fig. 3 wird ersichtlich, dass in der 13.–14. Woche der Lehrling zum Durchschnittsakkordverdienst kommen wird, sofern die Prognose seiner Entwicklungsfähigkeit und das vorgesehene Übungsgesetz der Maschinenbedienung richtig gewesen sind. Besonders beachtlich sind die Umschulungsergebnisse langjähriger Arbeitsloser, deren Wiedereingliederung in die Wirtschaft in der Regel Schwierigkeiten macht. Der Facharbeiter, der 4–5 Jahre arbeitslos gewesen ist, verliert nicht nur an Geschicklichkeit um 30–40%, sondern darüber hinaus auch an allgemeiner Einpassungsfähigkeit in eine Arbeits- und Leistungsordnung, wie sie für jeden Betrieb unerlässlich ist. Nur durch planmässige und

individuelle sowie pflegliche Schulung wird eine zwar geminderte, wenn auch immerhin wirtschaftlich erträgliche Arbeitsleistung auch in diesem Falle in Aussicht gestellt werden können. Mit dem Erliegen einzelner Industriezweige und dem gleichzeitigen Aufblühen neuer, teils anders gearteter, hat die industrielle und Wirtschafts-Psychologie neue und sozial wichtige Umschulungsaufgaben erhalten. Handelt es sich doch darum festzustellen, welche Berufe sich für die Umschulung für eine neue Tätigkeit bestimmter Art besonders eignen, welche Altersklassen besonders und welche weniger geeignet sind, in welcher

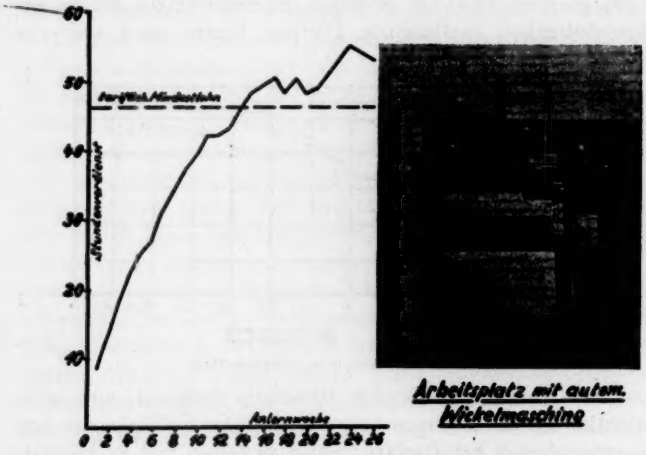

Fig. 3. Anlernkurve beim automatischen Wickeln

Weise für das Arbeitsschicksal eine frühere Lehre und eine frühere Betätigung auf einem anderen Arbeitsgebiete oder eine kürzere oder längere Arbeitslosigkeit bedeutungsvoll sind.[4] Die theoretisch psychologischen Übungsgesetze haben damit eine breite praktisch wirtschaftliche und soziale Grundlage erhalten und es wurden neue und wertvolle Einsichten in das Wesen der Übung und Schulung von Fähigkeiten, Fertigkeiten und Verhaltungsweisen vermittelt.

Arbeitstechnik

Unter Arbeitstechnik verstehen wir die auf Sicherheit, Wirtschaftlichkeit und Wohlfahrt gerichtete Bestgestaltung der Arbeitsverfahren eines Menschen in einem Betriebe.

[4] E. Hänsgen, Gedanken über betriebsnahe Schulung, *Industr. Psychotech.*, 13, 1936, 225-233.

Erste Aufgabe der arbeitstechnischen Studie ist es, die Unfallgefährdung im Betriebe und der Wirtschaft zu verringern oder zu beseitigen. Den technischen Unfall-Verhütungsmitteln sind die psychologischen anzureihen, die mit den Hilfsmitteln der Eignungsfeststellung, der Schulung und des Trainings besonders unter Gefahrbedingungen sowie der Werbung für unfallsicheres Verhalten arbeiten.

Der unfallgefährdete Menschen, besonders im Verkehrsbetriebe, kann in der Regel durch Eignungsprüfung erkannt werden, so dass seine Warnung und Fernhaltung von gefahrbedrohter Arbeit möglich ist.[5]

Für die Unfallverminderung im Verkehrsleben und im Kampf gegen den Verkehrstod erwachsen durch Einführung der obligatorischen Alkoholprobe neue Aufgaben. Nach der deutschen Reichsstrassen-Verkehrsordnung ist jeder Fahrer zu bestrafen, der sich in einem Zustande geminderter Leistungsfähigkeit an das Steuer setzt. Bei einem Unfall werden bei Alkoholverdacht alle Beteiligten, der Fahrer des Kraftwagens, des Fahr- oder Motorrades, sowie die geschädigten oder beteiligten Fussgänger einer Alkoholblutprobe unterzogen, deren Ablehnung nicht statthaft ist.

Sache psychologischer Leistungsbegutachtung ist es festzustellen, ob und in welchem Masse durch den genossenen Alkohol verkehrswichtige Leistungen geändert und besonders herabgesetzt wurden. Zur Anwendung gelangen zunächst in jedem Falle: (1) die chemische Alkoholproben; (2) der Beobachtungsbefund des Polizeiarztes, dem sich die psychotechnischen Leistungs- und Verhaltensproben zugesellen können.

Die von Widmark[6] angegebene Umsatzformel, $A = p \cdot r \cdot (C_t + \beta_t)$, wird bei der chemischen Analyse befolgt, wobei p das Körpergewicht in kg., r den Verteilungsfaktor (Männer: 0,68; Frauen: 0,55), C_t die Konzentration im Blut in ‰, β die Umsetzungsgeschwindigkeit = 0,15/Stunde, und t = Zeit in Stunden darstellt.

Geklärt sind zur Zeit noch nicht die Grösse der von Widmark selbst angegebenen physiologischen Unterschiede, die je nach der Persönlichkeitseigenart zu um 30–40% verschiedenen chemischen Blutalkoholbefunden führen. Die Beobachtung des Arztes erstreckt sich auf eine Reihe von Gebrauchshandlungen und allgemeinen Verhaltungsformen des Alkoholbeeinflussten, die wenn möglich unmittelbar nach dem Unfallgeschehen vorgesehen sind.

[5] Moede, Unfäller und Nichtunfäller im Lichte der eignungstechnischen Untersuchung, *Med. Welt*, 6, 1935.

[6] Widmark, *Die theoretischen Grundlagen und die praktische Verwendbarkeit der gerichtlich-medizinischen Alkoholbestimmung*, 1932.

Die Schwelle, also die Empfänglichkeitsgrenze für alkoholische Beeinflussung, ist nach bisheriger psychotechnischer Erfahrung ausserordentlich verschieden, wenn auch bei bestimmter Grösse des zugeführten Alkohols schliesslich jeder Fahrer und Fussgänger für beeinflusst gelten kann. Die Eignung des Fahrers ist wesentlich für sein Alkoholverhalten, seine Fahrtechnik als erworbene und beherrschte Fahrgeschicklichkeit, der Zustand der Gesamtpersönlichkeit in körperlich-geistig-seelischer, charakterlicher Hinsicht, die Umstände der Gefahrsituation, mit denen Fahrer und Fussgänger in Wechselwirkung treten.

Wir fanden, dass sich mitunter bei Alkoholmengen von 0,8‰ im Blut schwere Beeinträchtigungen der Fahrdienstfähigkeit auf dem Prüfstand nachweisen liessen, die sich auch bei Probefahrten auf dem Wagen zeigten, bei denen schwierige Situationen, die geistesgegenwärtiges Verhalten erforderten, nicht mehr gemeistert werden konnten, wenn auch die mechanisierten Handlungen noch störungsfrei abliefen. Mitunter aber fanden wir, dass bei der doppelten Alkoholmenge von 1,6–1,8‰ im Blut der Fahrer mängelfrei sein Gefährt, auch bei schwieriger Verkehrslage, steuerte und mängelfrei auf dem Prüfstand reagierte, so dass eine individuelle Diagnose mit allen zweckdienlichen Mitteln zur Klärung gerade der kritischen Alkoholzone erwünscht ist. So richtig die allgemeine Forderung ist, vor Antritt der Fahrt und während der Fahrt keinen Alkohol zu geniessen, so notwendig wird der Nachweis im Einzelfall, ob der Alkohol für ein Unfallgeschehen kausal anzusehen ist.

Die Bestgestaltung der Arbeitsbedingungen ist nur auf der Grundlage von Arbeitsstudien möglich, bei denen alle am Arbeitsprozess beteiligten Faktoren des Menschen, Sinnesleistung, Aufmerksamkeit, die Bewegungen, die Persönlichkeitshaltung in Betracht zu ziehen sind.[7] Die von Taylor eingeführten Zeitstudien sind demnach zu allseitigen Arbeitsstudien fortentwickelt worden, um die arbeitspsychologischen Gesetzmässigkeiten der einzelnen Arbeitsverrichtungen zu erkennen, um Mängel am Arbeitsablauf zu beseitigen und Regeln der Bestgestaltung bei Neuordnung anzuwenden.

Optimale, d.h. griffgünstige Stellen für Bewegungen (Fig. 4), sind z.B. alle Griffe in Richtung von etwa 30° schräg rechts vom Körper, bei einer Arbeitsebene etwa in Ellenbogenhöhe. Wir sehen auf der Abbildung die zeitgleichen Grifforte und können für häufig wiederkehrende Griffe die griffgünstigsten aussuchen.

[7] Moede, *Arbeitstechnik*, 1935, 1-267.

Der arbeitstechnischen Bestgestaltung dienen insbesondere auch die Leistungs- und Ermüdungsmessungen, die mit Leistungsstatistik, Pausenstudien oder Differenzproben arbeitswichtiger Funktionen vorgehen. Als Beispiel für die optimale Pausengestaltung führen wir Studienergebnisse der Fig. 5 an. In einer Zuckerfabrik, bei der fortlaufend Einstellungen am Polarimeter zur Überwachung des Kochprozesses erforderlich waren, galt es, möglichst fehlerfreie Leistungen über 8 Stunden zu erzielen, was nur durch geeignete Pausengebung möglich war. Den geringsten Leistungsfehler in Höhe von f = 1,09

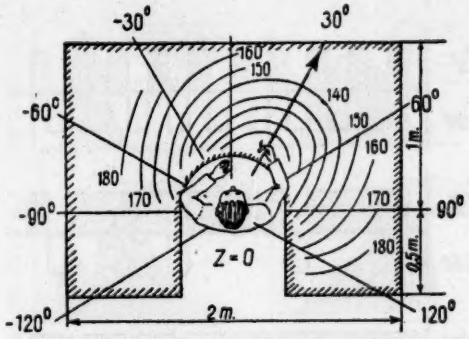

FIG. 4. BEWEGUNGSFELD
(Die Linien geben die Orte gleicher Griffzeit an.)

erhielten wir bei zeitgleichen Arbeits- und Pausengrössen, wo also nach 10 Sekunden Arbeit 10 Sekunden Pause folgten.

Die Zeitstudien Taylors trugen dem Anstrengungsfaktor der Arbeitspersönlichkeit nicht Rechnung, ist doch gleicher Leistungserfolg und gleiches Pensum bei verschiedener innerer Anstrengung des Arbeiters möglich und üblich.

Die Kennzeichnungswerte der menschlichen Arbeit sind in Tabelle III zusammengestellt.

TABELLE III

MASSE MENSCHLICHER ARBEIT UND LEISTUNG

1. Physikalische Masswerte
   im c-g-s-System
2. Physiologische Masswerte
   Stoffwechselmethode (CO^2, O usw.)
3. Arbeitstechnische Masswerte

   Arbeitsmass { Stück / Zeit

   Leistungsmass = Stück/Zeit

4. Arbeitswirtschaftliche Masswerte

Arbeitsertrag { Stück/Unkosten / Zeit/Unkosten

Leistungswirtschaftlichkeit = Stück/(Zeit × Unkosten)

5. Arbeitsseelische Kennzeichnung

Intensitätswerte, also Anstrengungshinweise geben besonders die mittleren Streuungen der Leistungszeiten, wie Fig. 6 zeigt.

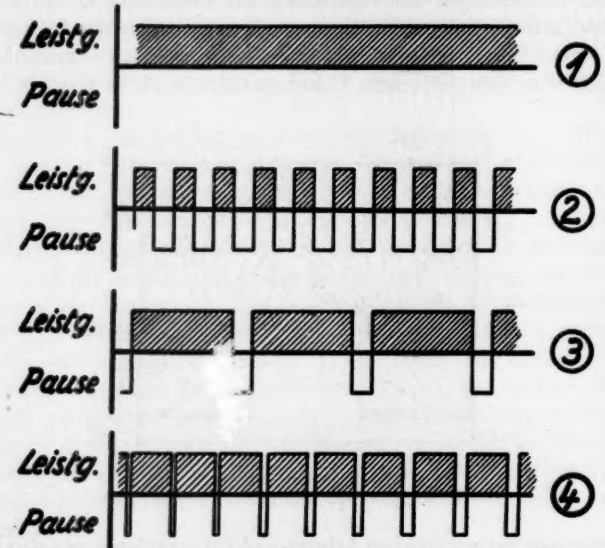

Fig. 5. Einwirkung von Arbeitspausen auf die Augenermüdung
Reiheneinstellungen am Polarimeter. Zeit je Einstellung 10 Sek.; n = Leistungszeit/Gesamtzeit; mf = mittlerer Fehler in 1/100 Grad.
(1) ununterbrochene Arbeit: n = 100%; mf = 1,33
(2) 10 Sek. Arbeit abwechselnd mit 10 Sek. Pause: n = 50%; mf = 1,09
(3) Min. Arbeit abwechselnd mit 1 Minute Pause: n = 83%; mf = 1,39
(4) je drei mal: 5 Min. Arbeit abwechselnd mit ½ Min. Pause
je drei mal: 5 Min. Arbeit abwechselnd mit 1 Min. Pause
je drei mal: 5 Min. Arbeit abwechselnd mit 1½ Min. Pause
je zwei mal: 5 Min. Arbeit abwechselnd mit 2 Min. Pause
n = 80%; mf = 1,12

Durch Einsatz und Vergleich verschiedener Kennzeichnungsmittel und -arten, kann allein Einblick in den objektiven und subjektiven Energieverbrauch bei verschiedener Arbeitsart und verschiedenen Arbeitsumständen gewonnen werden.

Konsumpsychologie

Der Massen- und Einzelkonsum kann sich ebenso wie der Produktionsprozess mit gutem Nutzen psychologischer Feststellungen und

Methoden zwecks Gewinnung konsumpsychologischer Kennwerte von Waren aller Art auf dem Markte bedienen. Von solchen konsumpsychologischen Kennwerten wollen wir einige anführen, nämlich den Bekanntheitswert, den Bekanntheitsnutzwert, die Geltungsart, den markttypischen Verkauf und den warentypischen Verbrauch.[8]

Bekanntheitswert. Der Bekanntheitswert drückt die Grösse der Bekanntheit eines Markenartikels in einem bestimmten Verbrauchergebiet aus. Er wird dargestellt einmal in der Bekanntheitsziffer, die uns angibt, wie viele von 100 Personen eines Konsumgebietes den Markenartikel kennen, zum anderen in der Schwankungsziffer, die uns sagt, um welche

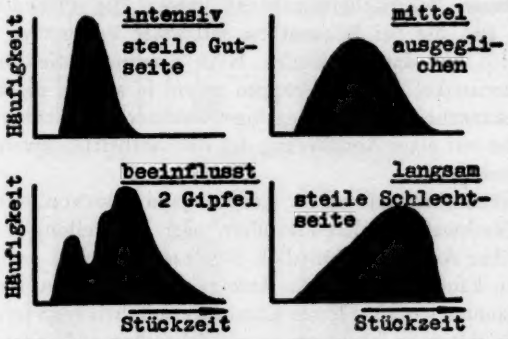

FIG. 6. INTENSITÄTSBEWERTUNG AUS DER HÄUFIGKEIT VON GRIFFZEITEN

Werte dieser Bekanntheitswert streut, wenn wir lediglich bestimmte Schichten von Personen und Verbrauchern in einem Gebiet berücksichtigen.

Die obere Grenze des Bekanntheitswertes liegt bei 100%, was bedeutet, dass von 100 befragten und berücksichtigten Personnen alle den Markenartikel kennen. Die Feststellung dieses Bekanntheitswertes kann nur methodisch erfolgen, um Irrtümer auszuschalten. Die Frageliste hat sich in vielen Fällen als nicht geeignet erwiesen. Durch methodische Kontrolle ist die Zuverlässigkeit der Bekanntheitsangaben zu erweisen.

Es sind nicht allzu viele Markenartikel in Deutschland, die diesen 100%-Wert erreichen, bewegen sich doch die Bekanntheitsziffern oft nur zwischen 60–80%. Die Schätzungen der Bekanntheit durch die Geschäftsleitung sind oft recht fehlerhaft.

Die Geschlossenheit, mit der die Werbung eines Markenartikels

[8] Moede, *Konsum-Psychologie*, 1933, 1-83.

durch alle Schichten durchgedrungen ist, drückt sich in dem Schwankungswerte aus. Es kommt in Deutschland oft vor, dass bestimmte Markenartikel, z.B. in Arbeiterschichten, einen 90%-igen Bekanntheitswert haben, während sie in der Schicht der höheren Angestellten nur zu 20–30% bekannt sind.

Die Markenartikelfirmen erstreben grösstmögliche Bekanntheitsgrade bei geringster Schwankungsbreite.

Bekanntheitsnutzwert. Unter dem Bekanntheitsnutzwert wollen wir den durch Konsumnachweis sowie konsumpsychologische Feststellungen zu erweisenden Nutzen verstehen, den die Firma aus ihrer Bekanntheit zieht. Dieser Bekanntheitsnutzwert drückt die Prozentziffer der Personen aus, die bei Bekanntheit mit einer bestimmten Ware sie gelegentlich oder dauernd kaufen. Nach unseren Studien müssen wir die Markenartikel in zwei Gruppen teilen: in solche, die über einen hohen Bekanntheitswert, aber geringen Bekanntheitsnutzwert verfügen und solche mit einer Annäherung des Bekanntheitsnutzwertes an den Bekanntheitsgrad.

Die Bekanntheit mit einem Artikel kann also von 100 Personen gemäss Nachweis der Konsumziffern oder Feststellung konsumpsychologischer Art durchschnittlich 30 veranlassen, den Artikel gelegentlich zu kaufen, und 15, ihn dauernd zu beziehen, so dass bei den Letztgenannten von einer festen Konsumgewohnheit gesprochen werden kann, die mitunter jahrelang, ja ein Jahrzehnt und mehr, bestehen kann.

Die Beobachtung dieses Bekanntheitsnutzwertes ist für den Geltungsgrad des Markenartikels als eines lebendigen im Urteil des Marktes sich dauernd entwickelnden Wertes recht bedeutsam. Die Bekanntheit besteht oft noch jahrelang fort, während der Nutzwert, also die Zahl der Dauerkunden, sinkt und sinkt. Die Beständigkeit der Qualität kann zu einer Dauerwertschätzung bestimmter Eigenschaften des Markenartikels führen, während freilich auch Ermüdungserscheinungen auftreten können, so dass neu auf dem Markte erscheinende Waren, sofern sie auf diese Ermüdungserscheinungen und das vielfach vorhandene Abwechselungsbedürfnis in geeigneter Weise Rücksicht nehmen, einen wachsenden Personenkreis gelegentlicher oder dauernder Konsumenten erhalten. Grosstadt und Kleinstadt verhalten sich anders, desgleichen sind die Konsumgewohnheiten in den einzelnen Teilen und Gegenden Deutschlands von recht verschiedener Empfindlichkeit und Festigkeit.

Geltungsart. Unter Repräsentanz oder Geltungsart verstehen wir die

Art und Weise, mit der ein Markenartikel Wirksamkeit im Bewusstsein, im Denken, Fühlen und Handeln der Konsumenten gewonnen hat.

Von besonderer Wichtigkeit ist die erste Stellungnahme sowie die erste Reaktion, die beim Sehen, Hören eines Markenartikelnamens eintritt. Diese erste Stellungnahme und Reaktion kann von einem "good will" des Publikums zur Firma und ihrem Erzeugnis zeugen, jedoch auch in gefühlsneutraler Haltung gelegen sein oder sich in negativen Einstellungs- und Verhaltungsweisen äussern, die sofort in Worten, Handlungen oder Gesten zum Ausdruck kommen. Geschäfts-

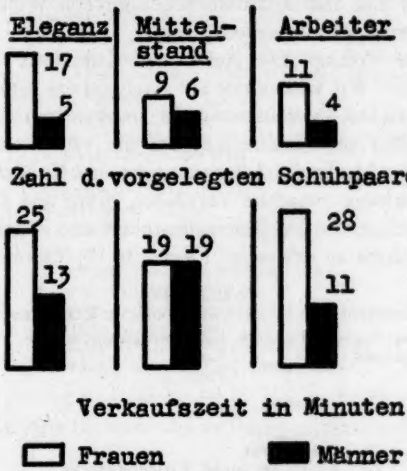

FIG. 7. SCHUHVERKAUF; ZAHL DER VORGELEGTEN SCHUHPAARE UND VERKAUFSZEIT IN MINUTEN
(Geringste Zeit der Schuhverkauf 3 Min.; längste Zeit 58 Min.)

wichtige und geschäftsnützliche, unwillkürlich einsetzende Erstreaktionen sind notwenig und zu sichern.

Der erste Gedanke, der beim Nennen des Markennamens anspringt, sollte die Ware und ihre Eigenschaften betreffen. Nicht nur der Inhalt dieser ersten Vorstellungen und Gedanken, die ohne unser Zutun aufkommen und die wir nur festellen können, ist wichtig, sondern auch die für die Reaktivierung dieser automatischen Gedankengliederkette erforderliche Zeit. Je sicherer und fester ein Tatbestand im Kopf des Konsumenten verankert ist, je geläufiger und reibungsloser der Fortgang von einem Begriff und Gedanken zum anderen, um so besser ist dies für die Marktgeltung des Artikels. Man wird auch die Umkehr-

probe machen und eine bestimmte Ware, etwa Oel, Sekt, Auto nennen, um zu ermitteln, welche Marke bei der durchschnittlichen und überwiegendem Zahl der Befragten genannt wird, wobei gleichzeitig die Zeitdauer vom Nennen des Stichwortes bis zur Antwort mit zu bewerten ist.[9]

Bedeutungsvoll sind beispielsweise auch der Bekanntheitsgrad, der Wirkungsrückstand und die Geltungsart der verschiedenen Werbearten, die mit verschiedenem Kostenaufwand im Jahresfeldzug laufen und gelaufen sind. Es zeigen sich oft starke Missverhältnisse zwischen dem Kostenaufwand und dem konsumpsychologischen Wirkungswert und Nutzen bestimmter Reklamearten.

Markstypischer Verkauf. Die Reihe der Kennwerte kann beliebig vermehrt werden. Wir wollen nur auf zwei weitere hinweisen: einmal den markentypischen Verkauf sowie den markentypischen Verbrauch.

Verkaufsstudien sind anzusetzen, um die typischen Bedingungen beim Einkauf der Marke durch Interessenten und Kunden zu ermitteln. Die Wechselwirkung zwischen Verkäufer, Ware und Kunden gilt es bewusst zu machen, um die Gesetzmässigkeit und Regelhaftigkeit des Verkaufsgeschehens zu erkennen. (Vgl. Fig. 7, Tabellen IV und V).

TABELLE IV

VERKAUFSSTUDIEN—BEOBACHTUNGSBOGEN BEI KLEIDERKAUF

Datum: Tageszeit; Intensität; Kaufdauer; gekauft, ja nein

Kaufprozess:
- Kaufwunsch: { Art / Preis
- Kauferfolg: { Art / Preis
- Klärungsart
- Klärungszeit
- Anzahl der vorgel. Kleidungsstücke
- Anzahl der angezog. Kleidungsstücke
- Bedienungsanteil:
- Bedienungsperson:
- Absteckerin:
- Aufsicht:

Verkäuferin: (Eigenart und Verhalten)
Kundin: (Eigenart und Verhalten)
Begleitung: (Eigenart und Verhalten)
Welche Schwierigkeiten lagen hier vor?
Wie wurden sie behoben?
Gründe für den Kauf (für die Pleite)
subjektive: objektive:

TABELLE V

DEUTSCHE KUNDENTYPEN

I. Tempo und Temperament:
1. Langsame—Schnelle, Bewegliche, Hastige
2. Einsilbige, Wortkarge—Redselige,
3. Schwerfällige—Frische, Lebhafte

[9] Moede, *Experimentelle Massenpsychologie*, 1920, 1-239.

II. Gefühlsart
1. Ernste—Heitere
2. Missmutige, Unlustige, Mürrische—Lustige,
3. Ruhige—Reizbare, Nervöse
4. Geduldige—Ungeduldige
5. Stille—Gefühlsüberschwängliche

III. Willensart:
1. Unselbständige, Hilflose—Selbständige
2. Passive, Zaghafte—Aktive, Energische, Draufgänger
3. Ablenkbare—Konzentrierte,
4. Unentschlossene, Unentschiedene, Unsichere, Schwankende—Entschlossene, Zielbewusste, Bestimmte

IV. Geistesart:
1. Dumme—Intelligente, Urteilsfähige
2. Unklare, Planlose—Klardenkende
3. Kritikaster, Bedenkliche, Kleinliche, Pedanten, Umständliche, Verbohrte—Sachliche, Einsichtige
4. Altmodische, Hinterweltler—Moderne, Fortschrittliche.

V. Soziales Verhalten:
1. Anspruchsvolle, Überhebliche—Bescheidene, Schüchterne
2. Freche, Feindlich-Schroffe—Freundliche, Liebenswürdige
3. Verschlossene—Mitteilsame
4. Nörgler, Trietzer, Dankbare
5. Unbeholfene—Selbstsichere
6. Affektierte—Schlichte
7. Unordentliche, Sorglose—Ordentliche.

VI. Kleidungsart:
1. Aufgeputzte, Gezierte—Einfache, Solide
2. Moderne—Altmodische
3. Korrekte, Saubere, Gepflegte—Saloppe, Unsaubere
4. Auffällige—Unauffällige
5. Ländliche, Provinzlerische—Grosstädtische
6. Kultivierte—Gewöhnliche.

Die Beobachtung und Analyse hat sich vom Betreten des Ladens durch den Kunden bis zum abgeschlossenen Kauf oder Nichtkauf zu erstrecken.

Derartige Studien führen zu bestimmten Bestformen der Warenvorlage, der Wunschklärung, der Gesprächsführung und des Verkaufsabschlusses, die durch geeignete Schulung und Überwachung in ihrem Bestande und ihrer Geltung zu sichern und zu verbessern sind.

Warentypischer Verbrauch. Die Verbrauchsanalyse soll alle Bedingungen ermitteln beim Verbrauch der eingekauften Ware. Die Arbeiten der psychologischen Konsumforschung führen fast stets zu bedeutungsvollen Anregungen über Verbesserung der Marke, ihrer Packung, der Werbung, der Argumentierung beim Verkaufsgespräch. Die einzelnen Verbrauchertypen konsumieren je nach ihrer Eigenart ganz verschieden, und sie äussern bei derartigen Studien die verschiedensten Verbesserungs- und Änderungswünsche der von ihnen geschätzten Marke. Auf die Mängel und Hemmungen, die genannt werden, sollte man stets eingehen um sie nach Möglichkeit abzustellen oder um angebliche

Mängel als Vorurteile und Missverständnisse aufzuklären. Derartige Konsumstudien stellen eine Parallele zu den Arbeitsstudien in der Produktion dar, wenngleich sie nach anderen Methoden erfolgen.

Sonntagsausflug
nach Hamburg.

Am Sonntag, den 14. Juni 1925

Sonntagssonderzug 4. Klasse nach Hamburg.

Besichtigung von Hagenbecks Tierpark.

Fahrkartenverkauf:

Besondere Bestimmungen.

FIG. 8. BEISPIELE VON BEACHTUNGSPRÜFUNGEN

Es ist Sache praktischer Konsumpsychologie, in strenger und erprobter Methodik und durch Massnahmen der verschiedensten Art: Statistik Befragung, Beobachtung, Einfühlung, kontrollierende vergleichende Analysen, Erfolgskontrollen, einen Konsumsachverhalt aufzuhellen. Die unparteiische konsumpsychologische Analyse kann ergänzt werden durch die Anschauungen und Feststellungen der kaufmännischen

Stellen. In klärender Aussprache sind die verschiedenen Feststellungen und deren Auswertungen miteinander abzustimmen. Mitunter werden dann neue Verfahren in Gang gesetzt, etwa Probeverkauf nach planmässiger Vorbereitung, Probegespräche, Anlernung und Schulung u.a.m. Auf dem Gebiet des Werkstattlebens, also der Gütererzeugung,

FIG. 9. BEISPIELE VON BEACHTUNGSPRÜFUNGEN

sind in Deutschland Zeit-, Leistungs- und Arbeitsstudien zu einer Selbstverständlichkeit geworden, während Konsumstudien als Markt-, Verkaufs- und Werbungsanalysen wesentlich geringere Verbreitung gefunden haben.

Werbsachenprüfung. Die Richtlinien der Werbsachenprüfung haben sich den Sonderbedingungen des Einzelfalles anzupassen. Zunächst wird der gesamte Wirkungsrückstand der Werbung einer Firma auf dem Markte zu ermitteln sein, um ein Urteil über die voraussichtliche Zusatzwirkung der neuen Werbsachen und des neuen Werbeplanes zu

bekommen. Die Begutachtungsverfahren sind recht verschieden. Bald handelt es sich um Beachtungsprüfungen des Inhaltsbestandes eines Plakates oder Inserates, bald um eine Werturteilsstatistik, um die wertende Gesamtstellungnahme des Beschauers kennen zu lernen, bald ist die Auffälligkeit, der Gedächtniswert, die ästhetische Seite gesondert zu studieren. Im Mittelpunkt derartiger psychologischer Begutachtungen steht die Beleuchtung der Willenswirkung, also des Einflusses der Werbsachen auf Kaufneigung und Kaufentschluss.

Erfolgskontrollen, so schwierig sie auch durchzuführen sind, sind fortlaufend anzustellen, um die Leistungsfähigkeit der Begutachtungsverfahren kennen zu lernen und zu verbessern.

FIG. 10. BEISPIELE VON BEACHTUNGSPRÜFUNGEN

Beispiele von Beachtungsprüfungen des Inhaltsbestandes eines Plakates zeigen Fig. 8 und Tabelle VI, während Fig. 9 und Fig. 10 sowie Tabelle VII ein Beispiel einer Gesamtbewertung zweier im Wettbewerb miteinander stehenden Plakate der Unfallverhütung wiedergeben.

TABELLE VI

BEACHTUNGSSTATISTIK DES PLAKATES "SONNTAGSAUSFLUG NACH HAMBURG"

Inhalt	Beachtung
Sonntagsausflug	47,0%
nach Hamburg	100,0%
Am Sonntag den 14. Juni	11,7%
Sonntagssonderzug	29,4%
4. Klasse nach Hamburg	35,3%
33⅓%	29,0%

Ermässigung	47,0%
Hafenrundfahrten	76%
Besichtigung v. Hagenbecks Tierpark	64,7%
Besondere Bestimmungen	35,3%
1. Der Sonderzug darf nur mit den . . .	
2. Kinder im Alter von 4-10 Jahren . . .	

TABELLE VII

VERGLEICHENDE WIRKUNGSSTATISTIK VON UNFALLVERHÜTUNGSBILDERN

	Bild I	Bild II
I. Dargestellter Vorgang		
a. richtig erfasst	64,3%	90,2%
b. nicht ganz richtig erfasst	8,3%	2,2%
c. nicht richtig erfasst	27,4%	7,6%
II. Beschriftung		
a. richtig wiedergegeben	58,3%	81,5%
b. nur halb richtig wiederg.	23,8%	0
c. nicht wiedergegeben	11,9%	12,0%
d. nicht richtig wiedergegeben	6,0%	6,5%
III. Zweck des Bildes		
a. richtig erfasst	90,6%	94,7%
b. nicht richtig erfasst	9,4%	5,3%
IV. Stimmungsgehalt		
a. Stimmung der Vorsicht	23,3%	31,8%
b. keine Stimmung	30,3%	29,5%
c. Mitgefühl	20,9%	6,8%
d. Angst, Bedrücktheit	18,5%	29,5%
e. verfehlte Antworten	6,9%	2,3%
V. Werturteil über das Bild		
a. Zustimmung zu dem Bild	58,5%	57,9%
b. bedingte Zustimmung wegen Ablehnung v. Einzelheiten	5,1%	14,0%
c. bedingte Zustimmung wegen Ablehnung. v. Unfallverhütungsbildern überhaupt	3,7%	3,5%
d. Ablehnung des Bildes	28,3%	15,8%
e. Ablehnung v. Unfallverhütungsbildern	0	5,2%
f. Urteilsenthaltung	3,7%	3,5%

In Deutschland ist die Werbung in den letzten Jahren neue Wege gegangen, bei denen das Prinzip der Wahrhaftigkeit und Lauterkeit im Vordergrund steht.

Die Werbung des Einzelhändlers, sei es nun ein Inserat, ein Plakat, eine Schaufensterauslage, wird auch nach der volkswirtschaftlichen Seite beachtenswert sein, da die Verbrauchsregelung und -lenkung eines gesamten Volkes ebenso wie die staatliche Betreuung seiner Fabrikation von staatspolitischer Wichtigkeit sind. Dienende und helfende Tätigkeit im grossen Ganzen des Staats- und Wirtschaftsgeschehens zu leisten, sind praktische und angewandte Psychologie sowie schliesslich alle angewandten Wissenschaften berufen unbebeschadet der Eigengesetzlichkeit ihrer Forschungs- und Feststellungsmethoden.

HINDU PSYCHOLOGY: PHYSIOLOGICAL BASIS AND EXPERIMENTAL METHODS

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(Founded in 1926)

The object of this paper is twofold. The first is to delineate some of the ideas of the ancient Hindus regarding the gross material basis of mental life and also the principles of its working, and the other is to discuss the probable physiological basis of the methods which they discovered and formulated for the development of the mental faculties. According to Hindu ideas, *mind* is a type of very fine material; it must have a *soul* to depend on and a body to live in. When oscillations and fluctuations of the mind are annihilated by special training, the mind becomes unified with the soul and can then know the true nature of anything to which it is directed. The Hindu experimental psychology chiefly concerned itself with devising methods for calming the fluctuations and oscillations of mind. It developed other special methods also for certain special purposes.

The ancient Hindus believed that the soul survived after death and in order to reap the fruits of good and bad deeds it is forced to take births. This is the doctrine of *Karma*. The ancients were also convinced that this process of transmigration can only be put an end to by the acquirement of true knowledge. It was the knowledge of the soul and the mystery of the world that can destroy the operation of the inexorable law of *Karma*. The aim of all the Philosophies was to discover the ways and means of attaining that knowledge.

All the different systems of Indian philosophy had this common aim and it was on psychology that they all relied for a true knowledge of the soul and for the analysis and classification of the entities of the phenomenal world.

Intellectual understanding takes one a very long way but that they felt was not enough. For a perfect understanding the nature of the mind has to be realised, hence the importance of the yogic methods in Indian philosophy, religion and psychology.

In ancient Indian literature psychological facts, theories and dis-

cussions are generally found in books and commentaries on philosophy, medicine, poetics, erotics, *Yoga* and *Tantra*. The whole super-structure of Indian philosophy and metaphysics is based on psychology. Indian science and philosophy relied on a particular physical theory of evolution of gross matter, from the five primary kinds of very fine matter, viz., *Kshiti* or 'earth,' *Ap* or 'water,' *Tejah* or 'fire,' *Marut* or 'air,' and *Vyoma* or 'sky.' The conception of these elements was arrived at by ancient sages not by any chemical methods but by deep introspective analysis of the excitability of the sense organs by external energies. Of the philosophies, the Samkhya system seems to have been the earliest. The Samkhya formulated a system of epistemology based on psychology, which profoundly influenced other systems. The theory of evolution appears closely to follow the stages of psycho-genesis of the human baby.

The Samkhya school believed primarily in two kinds of entities, matter and spirit. The former includes what modern psychology understands by mind and the latter is *Purusha* or soul which takes different bodies in successive births. Both were uncaused and indestructible. The spirit only was conscious and suffered no change but matter was non-conscious and always subject to changes of form, due to changes of state brought about by the automatic operation of the *Gunas* (properties, aspects or attributes). The phenomenal world evolved out of the initially unmanifest *Avyakta*, which is also termed the *Prakriti* or *Pradhana* or primordial matter or nature. The *Gunas* are three in number, viz., the *Sattva*, the *Rajah*, the *Tamah*. From the physical aspect they mean stable existence, attraction and repulsion respectively. They sway every form of matter including the mind, each dominating the situation in cyclic order.

But this evolution could not take place without the contact of the conscious spirit with the primordial matter. From the contact of the spirit and matter evolved a substance called *Mahat* or *Buddhi* or consciousness—vague, pure and simple—or intelligence. From this first evolute or *Mahat* is evolved the second, the *Ahamkara* or self apperception whence comes the sense of the self or ego which later becomes involved in all subsequent operations. From *Ahamkara* are developed the *Tanmatras*—the rudiments of the elements—the subtle essences of all formal existence. These cannot be particularised any farther than as vague mere somethings. Physically speaking they are the atoms (*Paramanu*) of five primary substances. These vague mere somethings appertain to the various energies that affect beings objectively as sound, contact, form, taste and smell. They are technically known as the

mere vague sound something (*Shabda Tanmatra*), the mere vague contactual something (*Sparsha Tanmatra*), the mere vague form something (*Rupa Tanmatra*), the mere vague tasty something (*Rasa Tanmatra*), and the mere vague olfactory something (*Gandha Tanmatra*). They are collectively called the five *Tanmatras*. On the psychical side there follow the evolution of *Indriyas* or inner sense organs for the apprehension of the sound, contact, form, taste and smell and they contain within them the substances they apprehend. The sense organs are the ear, skin, eye, tongue and nose respectively. These are called five organs of knowledge or *Jnanendriya*. Then follow the organs of action or *Karmendriyas*—*i. e.* the organ of speech, the hand, leg, rectum, and the organ of micturition and coitus—and then the internal organ—*Manah*—the mind.

From the *Tanmatras* which are regarded as the elements in their simplest form, evolve the fine and gross *Akasa* or *Vyoma* (sky) apperceived by the ear as sound, *Vayu* or *Marut* (air) apperceived by the skin as touch, the *Tejas* (fire) apperceived by the eye as form or colour, *Ap* or *Jala* (water) apperceived by the tongue as taste and *Kshiti* or *Bhumi* (earth) apperceived by the nose as smell. These are not to be regarded as what their names usually mean. They are the elements which in different combinations make up all formal existence the whole of the infinitely diversified phenomenal world that we can ever know. The things have now gained in content and have become specific objects. The understanding of these categories and their realisation through Yogic methods lead to true knowledge and freedom from birth. It is the knowledge of body, mind and soul and their inter-relation which is wanted.

The Hindu classification of matter rested on the receptivity of the different sense organs. The discussion of the concept of the properties of the five kinds of matter will lead us into metaphysical questions which we do not propose to pursue. The special property of *Kshiti* was smell, in addition to the other four properties. That of *Ap* was taste, in addition to the other three, that of the *Tejah* was form, in addition to the other two, that of *Marut* was contact in addition to one other; while the special and only property of *Vyoma* was sound. It would seem that there was condensation and augmentation of the properties possessed by mere attenuated substances. It should be noted here that according to the Hindu theory the sense organs themselves contain within themselves the various substances which they apprehend from outside. This may be called the Resonance Theory.

The Concepts of the Mind and of the Gunas

It is hardly possible to do justice to the vast amount of literature that is devoted to the concept of the mind. The basic ideas are noted below.

Mind: Material. The *Rishis* thought that mind was made up of and nourished by the finer material assimilated out of the food-stuffs that a man consumed. In the *Chhandogya Upanishadu*[1] is recorded an experiment, which is supposed to prove that mind is material in nature like the body. The father asked the son to verify this by going on fasting for fifteen days and reporting the results of his experience at the end so far as the mental activity was concerned. At the end of that period the son reported almost a total cessation of mental activity, interest, memory and cerebration. Anything "which is born, exists, grows, decays, changes, suffers fatigue and dies," *i.e.* shows these six kinds of manifestation, is known as a '*Bhava*' or material substance. Under this dictum, mind is material.

The word *Manah* or mind is used in two senses. One is attention and the other is the thinking principle in its totality. In the latter sense there is emphasis on the cognitive aspect. It has a propensity for acquiring knowledge with consequent constant oscillation from point to point. The stream of consciousness and memory is broadly expressed by the Sanskrit term *Chittam.* The word *Chit* means conscious. The *Chitta* is like a mirror on which images fall. The *Chitta* has two aspects. The primary one is the *Buddhi* or intellect. The function of *Buddhi* is to arrive at a decision as to the nature or classification of the impressions received as also the course of action to be adopted in the matter. This decision is arrived at after passing through a phase of doubt. The second aspect which ontogenically develops at a later stage is known as the *Ahamkara*—the ego, self consciousness, the idea of 'I' involved in every mental operation.

Mind: Antah-Karana. Mind is also known as the '*Antah-Karana*' or the inner *Indriya* or organ within. In this sense mind is sometimes for etymological reasons identified with the heart, where affective states and the ego-feeling are usually located. Mind is the eleventh organ presiding over the other ten consisting of two groups, the five *Indriyas* for knowledge, viz., the inner five sense organs for cognition of sound, contact, form, taste, and smell; and the five *Indriyas* for action, viz.,

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[1] *Op. cit.*, Chap. 6, par. 5-7.

those for speech, movements of arms, legs, defecation and coitus. *Manah* is the charioteer of the ten sense organs.

Mind: Apparently conscious. Mind is material and is not really conscious but is *Chidabhasa, i.e.* apparently conscious. It brings about the contact of the soul or the conscious entity on one hand, and the inner sense organs on the other. The mind derives its light of consciousness solely from the contact of the conscious entity—the *Purusha* or soul.

The word *Sattua* literally means the nature of what exists. The word *Rajah* means the tinging of or colouring of something by another while the word *Tamah* means darkness or cover. The bodily and mental processes are subject to the fundamental law of activity and rest or inhibition in some form of rhythm. The activity or work phase in the mental sphere is differentiated into two phases: (1) a stimulus or apperception-phase; and (2) the subjective response or reaction-phase of consciousness. The apperception and the response are analytically the image and the effect.

The operation of the three Gunas on mind. The objective impressionability of the mind leads to the development of the affection or feeling or an action attitude. The subjective response of affection may be either pleasantness, *i.e.* the craving for the continuance of the stimulus situation, or unpleasantness, *i.e.* repulsion towards it. The wish factor is at work behind affects. This is followed by the second phase of rest. Thus we have a scheme in which (1) apperception of the stimulus, (2) response or reaction, and (3) rest or inhibition or displacement have sway on mind in cyclic order. These are in my opinion the three *Gunas* which influence all the activities of creatures. The apperception of the stimulus is to be equated to the *Sattva;* the response or reaction of affective nature which generally follow, in the trail of pleasure and pain with wishes and activities, to *Rajah;* and lastly, the rest or inhibition or displacement, to *Tamah. Sattva* is pure consciousness of the true nature of things. *Rajah* is the affective state depending on the state of the mind at the time of perception, evoking changes due to the stimulation, while *Tamah* is cessation or cover of consciousness or an experience of drowsiness, sleep and similar states. It obstructs both the *Sattva* and the *Rajah.* This third state of rest which follows any work or stimulation is an automatic process renovating the ability of mind for further work.

Physical Basis of Mental Life

The humoral theory. This is no place to discuss the conceptions of Hindu physiology but a brief reference to the principles working the bodily

apparatus is necessary for an understanding of the practical methods of psychology. The humoral theory of *Vata* or *Vayu* (wind), *Pitta* (bile), and *Kapha* (phlegm) was resorted to explain all normal and morbid bodily and mental processes. Each of these principles was used in two senses—a gross and a subtle. Thus the *Vata* or *Vayu* stood for air and a driving force or dynamic principle underlying those processes which are now termed nervous activity in modern physiology. The *Pitta* stood for the gross bile and the chemical or cellular activity through the vehicle of various compositions specially of chromated particles in tissues producing body heat, digestion, cardiac movements, cardiotonus, vision, etc. The third the *Kapha* stood for phlegm and secretions, external and internal, and their metabolism. These three entities act in the body and interact on one another. The *Vata* or *Vayu* as the driving force dominates the working of the two other principles.

The importance of the head. The different tissues and organs of the body were built by the five primary types of matter as afore said: the earth, water, fire, air and sky. The bodily substrate of mental life was "the head (*Shirah*—the chief organ or part) which was so termed as in it the life of living beings and all the inner sense organs were located."[2] "Within the head are centred the sense organs and the *Srotas* (nerves) which are termed *Indriya Pranavaha*, *i.e.* those nerves which carry impulses to and from them, just as the rays are centred in the sun."[3] In the *Baghbat Uttara Sthana*,[4] in diseases of the head, there is a reference to the nervous system having its root up in the head and branches downwards. The comparison is very appropriate. The brain with the medulla forms the bulb which lies in the cranium pot, while the branches spread all over the body. The clear cut idea of a unified nervous system is not preserved in the medical literature traced to antiquity. A more or less detailed treatment of the parts are, however, generally available. There is a good description of the sympathetic chains—the *Ida* and *Pingala* on the left and right sides of the spinal cord or *Sushumna* which latter was well known as a conductor. The connections of the peripheral spinal nerves are not described. The knowledge of the nerves attached to sense organs in the head was there. There was considerable knowledge of the anatomy of the eye. It was known as a chemical sense from which impressions were carried to the brain by the activity of the

[2] *Charaka, Sutra Sthana*, Chap. 17, Kiyanath, Shirasiya.
[3] *Charaka, Siddhi Sthana*, Chap. 9, Trimarniya Siddhi.
[4] *Op. cit.*, Chap. 24.

Alochaka Pitta, or a phase of 'bile' function. This may be construed to mean the action of the photo-chemical substance in the eye.

The concept and functions of Indriyas. The *Charaka Samhita* discusses the sensations and mind and presents the following analysis.[8] "The vision, audition, olfaction, taste and contact are the five *Indriyas* or inner sense organs. The *Dravyas* or material objects of apprehension of the Indriyas are the sky or space, air, lustre, glow or fire, water and earth. The two eyes, the two ears, the two nostrils, the tongue and the skin are the *Adhisthanas*, seats of the *Indriyas*. The sound, touch, form, taste and smell are qualities of objects or *Vishaya*, *i.e. Indriyartha* of the sense organs. Contact of *Indriya*, *Indriyartha*, mind and the soul, produces five kinds of *Buddhi* or knowledge such as the visual, auditory, olfactory, gustatory and tactual perceptions. The said *Buddhi* is of two kinds: (1) the momentary, and (2) the confirmatory. The mind is in a sense above the *Indriyas*. It has its own objects of desire and has a tendency to seek pleasure. For the attainment of these it puts forth efforts. The efforts spring from the impressions derived in the sense organs. Though there are varieties of knowledge and impressions, the mind is not more than one, for mind cannot attend to more than one thing at the same moment. Mind is the controller and director of sense organs and organs of action and the *Indriyas* are able to apprehend their objects only when goaded by the mind. As both the *Indriyas* and *Indriyahisthanas* are located in the cranial cavity and as there is special mention of *Indriyapranabaha Srotas* radiating from the head and carrying impressions from the external sense organs, the *Indriyas* may be held to mean the specific cerebral centres—sensory and motor—of the modern physiologists.

Seats or centres. The seat of consciousness according to medical writers was the head containing the *Indriyas* or centres of the sense organs. But there are passages which locate mind, intelligence, consciousness, etc., in the heart. This seems to be an echo of the belief of the vedic times. It is perhaps the actual sensing of emotional disturbances in the region of the heart that led people to conceive of the heart as the seat of the consciousness. There was another reason. The heart is the centre of circulation and life and was naturally taken to be the centre of consciousness. The heart was also regarded as the seat of the ego-feeling or *Ahamkara*. The seat of the mind was located on the inner side of the junction of the eye brows.

The physical substrate of life and mind was operated by the *Vayus*,

[8] *Sutra Sthana*, Chap. 8.

which were ten in number, with different jurisdiction and functions. The principal ones were the *Prana*, *Udana*, *Samana*, *Vyana* and *Apana*.

Function of the five 'winds.' The *Prana* has its seat in the head, the thorax, the ears, the tongue, the mouth and the nose. Its functions are spitting, sneezing, eructation, breathing and deglutition. The seats of *Udana* are the regions near the navel, the chest and the thorax. Its functions are the utterance of speech, exertions and formations of energy, maintenance of strength and complexion. The *Samana* is located in the ducts that bear the sweat, the doshas (*i.e.* air, bile and phlegm) and watery juices. It imparts strength to what is described as digestive fire in the stomach and which produces body heat. The *Vyana* whose course is very swift pervades the body of man. Its functions are locomotion, the extending and contracting of the limbs, the sensation of touch, the winking of the eye lids. The two testes, the pubic region and the bladder, the thigh, groins and the anal canal are the seats of *Apana*. Seated in the intestine it expels (secretes) the vital seed, the urine and the stools. It is the 'wind' that expels also menstrual blood and the foetus from the womb. The 'wind' is the leader of the mind and the organs of sense and action. It would thus appear that the *Vayus* correspond to the physical energy of the nervous system.

Advances during the Tantric period. Apart from the discussions of medical writers, the physical basis of mental life received considerable attention in the hands of the *Hata Yogis* and particularly the *Tantric* worshippers in connection with the development of the *Kundalini Yoga*. They in their own way worked out in detail the functions of the brain, cerebro-spinal axis and the *nadis* (nerves) connected with it. They looked upon the whole system as the great reservoir of the power and energy which directed the operations of the whole body. Their description forces one to believe that they regarded the axis as the conductor and that the whole structure, from the *Muladhara*, *i.e.* the basis of the spinal cord, to the *Sahasrara* or the cerebrum, was endowed with consciousness which increases in degree as one approaches the cerebrum. There are six *Chakras* or centres on the same, which were posited near important plexuses and organs of internal secretions. The *Chakras* were the seats of various mental functions and also of the phonation of fifty elementary sounds or letters of Sanskrit alphabet.

Practical Experimental Methods

We are now in a position to explain the practical methods of the ancient Hindus for the understanding and realisation of the true nature

of the mind and the soul. There are systematic text books on these practical methods technically called the *Yogik* method. The word *Yoga* literally means the act of joining: joining of one thing to another. From the psychical standpoint the fixing of attention to any thing or idea is Yoga. The common word for attention in Sanskritic languages is joining of mind or *Mano Yoga*. The esoteric meaning is the joining of the mind to the *Atma* or *Purusha* or soul. It is well known that the mind is never at rest but oscillates from one object or idea to another, and the intensity of its activity fluctuates. This property of the mind or *Chitta* is known as *Vritti*. The essential condition for Yoga is, first of all, to curb and stop this property of oscillation. So in the most authoritative treatise, the *Patanjal*, Yoga is defined as the restriction of the oscillation of conscious mind. There is the assumption that when the *Chitta* is devoid of its *Vrittis*, the experiencer is able to know the true nature of his own soul or self. The Yoga system is recognised as one of the principal orthodox systems of philosophy originating at least 1500 B.C. It is dependent for its theoretical basis on (the categories of) the *Samkhya* system of which it may be said to be a practical counterpart. All the other philosophies accepted the truth and findings of the Yoga system. Though originally developed as a practical science for the metaphysical and religious purpose to devise means for the realisation of the categories of the *Samkhya* philosophy, it dealt fully with the various aspects of mind. The Yoga should be regarded as practical, experimental psychology which was discovered in India, and improved upon after considerable observation and experiment extending over long periods of time. Sages in ancient India and earnest votaries of different religious sects practised Yoga in one form or other; the latter do it even now, for the knowledge of the mind and the soul. According to them, by acquirement of knowledge of the soul and its realisation there is cessation of pain and rebirth.

The five kinds of Yoga. We shall now draw the attention of the reader to the great work of Patanjali, which has been translated into English several times.[6] This work is the most authoritative. It deals with *Raja Yoga*, the first kind of Yoga, which emphasises meditation and imagination. A second type of Yoga primarily stresses the physiological processes involved in cleaning the body through respiration, posture, etc., rather than through contemplation or concentration. This is

[6] The translation of Professor Woods of Harvard University is perhaps the latest and the most critical.

called technically *Hatha Yoga*.[7] The practice of the *Hatha Yoga* was done also with a view to minimize all the metabolic processes in the body and attain a control over the autonomic or involuntary nervous system and ultimately acquire supernormal powers in many directions. There is a third kind of Yoga called *Satchakra Bheda* or *Laya Yoga* or *Kundalini Yoga* embodied in the *Tantras* (esoteric works on various methods of religious worship). It emphasises meditation on certain *Chakras* or centres (most probably terminal plexuses of the autonomic or sympathetic nervous system) in the neck, thorax, heart and the abdomen in connection with the spinal cord and the lateral ganglion chains on its sides.[8] The word *Kundalini*, which literally means one which is coiled like a serpent, is the name of the force in the spinal axis which is sought to be activated. A fourth kind of Yoga is called *Mantra Yoga* which emphasises the effect of monotony of the speechless repetition of certain *Mantras*, or syllables or syllable clusters or formulas with certain meanings and believed to be endowed with considerable potency. The incantations or *Japa* lead to the subjugation of respiratory rhythm and development of eidetic imagery concerning the *Mantras*. The concentration to the dominant idea and kinesthesis thus brought about produces the desired result in time. *Bhakti Yoga* or devotion to and belief in the deity is another kind of Yoga and leads ulitmately to success.

The various methods of Yoga. The *Patanjal* deals with the methods of Yoga.[9] "Abstentions and observances and postures and regulation of the breath and withdrawal of the senses and fixation of attention and contemplation and concentration are the eight limbs (or steps) of Yoga." Remembering this aphorism, we now enumerate below the practical methods of attaining the Yoga. They involve physiological and psychological processes or more or less a combination of both.

(*1*) The first method is the purging of *mala* or impurities of body and mind of the neophyte. A healthy body free from physical impurities and mental disturbances was regarded as the first essential condition for the Yogic practice. The Hindus believed that the true knowledge cannot be acquired by one suffering from bodily ailments and from emotional disturbances due to wrong beliefs and shady moral make-up of

[7] The best known work on this subject is *Hatha Yoga Pradipika* by Swatmarama.

[8] The most well-known work on the subject is *Satchakra Nirupana* by Purnananda. This work has been translated into English in 1924 by Arthur Avalon under the name *Serpent Power*.

[9] *Op. cit.*, Chap. II, Sec. 29.

the personality. They further believed that the steps primarily conducive to the cleaning of the body go a long way towards cleaning the mind. There is a sort of transfer in the matter of cleaning which was believed in. On the physical side the process was known as *Satkarma* or six processes. They were as follows.

(*a*) The *Dhauti* is the washing of the stomach by means of a wet narrow piece of cloth twenty-two and a half feet long and about three inches broad. The art of swallowing the long piece is learnt by practice very slowly. The piece is taken out also very carefully. For a preparation of *Samadhi*, lasting twelve days or more, cleaning of the stomach should be as thorough as possible, so much so that the stomach is to be made free from any acid reaction. The reaction is to be tested by vomiting the milk drunk.

(*b*) The *Vasti* is the washing of the intestines by water, sucked or rushed by gravity through the sphincter of the anus by means of a bamboo tube of a calibre through which the little finger can pass, about four inches long, about two-thirds of it being inserted within. The man is to sit immersed in water up to the navel. The water is to be churned within from side to side by the process of *Nauli* and then purged out, when the tube should be taken out. It may be noted in this connection that the processes of *Dhauti* and *Vasti* should be performed before taking meals, and that meals should follow the processes immediately.

(*c*) The *Neti* is the cleaning of the two nostrils and the air passages by means of a soft string smeared with oil and about nine inches long. The process consists of sniffing one end through one nostril into the mouth and then by getting hold of the two ends moving the whole string to and fro. Gradually by practice the end remaining in the mouth can be expelled through the other nostril by expiratory efforts so that when both ends of the thread come out through the two nostrils they may be moved to and fro to clear the phlegm.

(*d*) The *Trataka* consists in attentively fixating on a small fine point without blinking until tears come out. The object should be a bright shining one for instance a small piece of pearl on black velvet. The man can stare into the eyes of another willing to coöperate and reciprocate the process. This process is for clearing the eye of impurities, but it is fraught with the capacity of considerable development of concentration and, if practised on the junction of the two eye brows, is said to develop clairvoyance.

(*e*) The *Nauli* consists of efforts at rolling the contents of the abdominal viscera from side to side in a posture when the trunk is a little bent forwards.

(*f*) *Kapala-bhati* consists in inhaling and exhaling breath without a break like the blacksmith's forge.

(*2*) The second process is psychological. It is the cleaning or purging the mind of its impurities. Unless the moral or emotional torpitudes of man are rectified, there is inhibition of intellectual powers and their functions. For the attainment of the necessary moral purity recourse was had to (*a*) *Yama* and (*b*) to *Niyama*.

(*a*) *Yama* or the five abstentions (*Patanjal*, Chap. 2, S. 30) are "the elements and degrees of morality in the form of prohibitons." Abstinence from (1) injury to others—*Ahimsa*, (2) falsehood—*Satya*, (3) theft—*Asteya*, (4) incontinence or freedom from sexual thoughts and acts—*Bramacharya*, and (5) acceptance of gifts—*Aparigraha*.

(*b*) *Niyamas* or five observances (*Patanjal*, Chap. 2, S. 32) are "advances in morality in the form of voluntary action." Observances of (1) cleanliness, external, bodily, mental and internal—*Soucha*, (2) contentment—*Santosha*, (3) austerities and self castigation, habituating the body to painful extremes of posture, temperature, etc.,—*Tapah*, (4) study of the Scriptures and of philosophy—*Swadhyaya*, and (5) devotion to God—*Isvarapranidhana*.

There is another principle which is formulated thus. "The mind is purified or freed from obstacles to concentration by the practice of the ideals of happiness, sympathy, pleasure, neutrality or indifference to others' pleasure, sorrow, good action and bad action respectively."[10] Still another may be recalled in this connection.[11] It is also very efficacious. "When there is the development of *Vitaraka*—where there is conflict in mind due to morally questionable ideas cropping up, such as jealousy, malice, rage, ill-will, etc., they are to be checked by efforts of developing opposite thoughts."

(*3*) The third method is mainly physiological. It consists in the adoption of certain definite postures, physical exercises and different methods of forcing air in desired channels. The first is the *Asana* (postures of various types). The second group, the *Mudras*, means combination of postures with certain physical exercises for the whole body, especially the contents of the abdomen. The third group, *Bandhas*, may be described as catches or contractions of different portions of the alimentary canal and sphincters to force the incarcerated air in a particular direction with the object of perforating the mouth of the *Sushumna* (the spinal cord). These *Asanas* and the *Bandhas* are specially stressed by the Hatha Yogis. A detailed discussion of these processes, the total number of which comes up to 100 or so, will lead us beyond the limits of the available space. They are described in many works such as the *Hatha Yoga Pradipika*, *Gheranda Samhita*, and the *Siva Samhita*.[12]

The *Asanas*, *Mudras*, and *Bandhas* were practised with a double object. The first was the maintenance of an easy posture for a very long time without causing any inconvenience or tendency to shift position. The second object was to modify the course of the normal physiological processes in the body activated by the *Vayu*, *Pitta* and *Kapha*, specially the first one. When the mind is restive there is frequent effort at changes of posture and vice versa and if any particular postures could be maintained for any length of time at will, mind comes to rest. That is to say the *Vrittis* or oscillations of the mind are then reduced to a minimum. Hence the emphasis was laid on the *Asana* as a preparation for the Yoga.

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[10] Patanjal, *Yoga Sutras*, Part 1, Sec. 33.

[11] *Ibid.*, Part 2, Sec. 33.

[12] Some photographs and pictures are published by Avalon in the *Serpent Power*. Cf. footnote 8.

Apart from these observations the Yogis discovered that every loss of balance or deflection from a particular *Asana* entailed loss of balance of respiratory rhythm, even though for a moment. Failure of a particular posture necessitates efforts of adjustment. Effort means increased respiration and increased consumption of oxygen. The *Asana* therefore helps the Yogi in deep stages of concentration to conserve oxygen. It seems that drawing on the reserve of oxygen in the system which would be inevitable in change of posture or movements is hardly permitted to a Yogi when he enters the stage of *Samadhi* needing 'almost absolute cessation of the respiratory and circulatory processes for a number of days or months.' The Yogis also believe that success in the practice of postures, etc. confers on one the power of suffering the extremes of temperature and of pleasure and unpleasure.[13] The best posture for an individual is to be found out after trials. The object of *Asana* is to obtain poise of the trunk so that the spine is straight and vertical and the centre of gravity does not shift from the sacral region. The muscles and tendons about the joints of the neck, the thigh and the knees should be placed under as small a strain as possible. When these conditions are attained and postures are practised, the disturbances of attention due to postural reflexes are minimised.

(*4*) The fourth method is also physiological in the main, but is often associated with concentration of attention. It is the regulation and restraint of breath. The ancients discovered the great principle that "when the breath flows the mind flows. When the former is quiet the latter is quiet and the Yogi becomes like a statue. Therefore the wind should be stopped."[14] In plain language when the frequency of respiration increases there is loss of balance of the respiratory rhythm and the oscillations of the thought increase. When the respiratory activity is reduced to a minimum, mental fluctuations are correspondingly reduced with it. *The Yogis discovered the inseparable association of the fluctuation of mind with the fluctuation of the respiratory processes.* In the same book, it is laid down as a dictum that "one who can stop the breath can stop his mind. By whatever process mind is stopped the breath is also stopped; if one can be stopped or controlled the other becomes stopped or controlled."[15] In the next passage it is declared that there are two causes of the fluctuation of mind. "One is the wish or craving *Vasana* or *Samkalpa*, and the other is the 'wind;' if any of these two is destroyed or annihilated both are destroyed." The Patanjal, discussing the methods of attaining concentration, speaks of "Expulsion and Retention of breath."[16] This process is called *pranayama* or the control of breath.

The *Pranayama* was held in very high esteem by the ancient Hindu. Its practice was obligatory with the three diurnal prayers. It was believed that it destroyed sins. *Pranayama*

[13] Patanjal, *Yoga Sutras*, Part 2, Sec. 48.
[14] *Hatha Yoga Pradipika*, Chap. 2, Sec. 2.
[15] *Op. cit.*, Chap. 4, Sec. 21.
[16] Patanjal, *Yoga Sutras*, Chap. 1, Sec. 34.

was regarded as a process of cleansing the body and the mind. The *Dharma Sutras* and the *Shastras* recommended the performance of *Pranayamas* for the expiation of many sins or acts of moral torpitude.[17] (The Hindus thought that the *Vayu* or Air through the respiratory process maintained the fire of the *Pitta* in the body. The fire gains expression in developing the body heat and digesting the food stuff. This fire, they thought, can burn away the impurities of the body and mind, like the impurities of metals. The tissues of the body were also termed *Dhatus* or metals according to a conception of Hindu medicine.) It was also believed that the *Pranayama*, when practised carefully, can obviate the necessity of the performance of the *Satkarma* or the six processes mentioned under (*2*). The Yogis thought that animals are generally endowed with a span of life measured by definite number of respirations. In man their number is given as 21,600 in the day or 15 in the minute. If the respiration is hurried life span is exhausted too soon, so the idea was to minimize the number of respirations per day. This would prolong life. One can thoroughly understand that owing to such beliefs, very great attention was paid to the respiratory process. In fact systems of very elaborate practices and theories developed regarding the nature of respiration and the method of bringing it under voluntary control.

The nature of the respiratory process has been discussed in what is called the *Svarodaya Shastra*. The Hindus noticed the alternation of respiration through the right and the left nostrils. It was observed also that during the transition, from one nostril to another, respiration was performed through both the nostrils for some moments and that a change in the direction of flow of breath can be brought about by several artificial means. If one lie on one side of one's body the respiration is performed through the opposite nostril. The same result is obtained if the axilla or the arm pit is pressed up by a horizontal piece of wood attached at right angles to a vertical one of suitable length. This mechanism is called the *Yoga-danda*. When the breath flows through the left nostril it is said to be flowing in the *Ida Nadi* or the 'moon' passage. When the flow is through the right side it is said to be flowing in the *Pingala Nadi* or the 'sun' passage. When the breath courses through both the nostrils it flows through the *Sushumna Nadi* or the 'fire' passage. We shall see later when discussing the *Kundalini* Yoga that these names of *Ida*, *Pingala* and *Sushumna* put on the breath flow are to be correlated to the two ducts on two sides of the spinal cord. The tubes (nerves) *Ida* and *Pingala* combine with the *Sushumna* in the neck just outside the cranium in the pharynx, so the air passages in the nostrils were regarded as if continuations of the same true nerves and the left and the right nostrils were thought to be continuations of the *Ida* and *Pingala*.

The normal order. The Yogis observed that in the healthy man there is a definite order of alternation of the breath flow from one side to the other. The average time for each side

[17] *Manu*, Chap. II, 200.

was about an hour and that "Commencing at day break with the first *Tithi* of the white fortnight of the moon for three days the breath flows through the left nostril, and in the case of the first three days of the black fortnights of the moon, the breath flows through the right nostril. The priority at day break is enjoyed by one side successively for three days, then its place is taken by the opposite side. It is believed if this sequence of alternation is not found in any case there is every chance of some mischief brewing. The Yogi is able to recognise the *Tithi* of the fortnight of the moon from a study of his own breath. In the white fortnight the breath flows in left and in black fortnight the breath flows in the right, and it should be the aim to attain the restriction of the flow through the right during the day and through the left during the night. And if the Yogi is able to control breath in this way he advances in the path of Yoga. The importance attached to the alternation of breath flow in the *Sarodaya* is generally recognised in the practice of *Pranayama*.

There is another aspect of the regulation of breath recognised by the principal works, specially of the Hatha school. Apart from the alternation of flow of breath there are three phases of respiration. Drawing in of air or inspiration was termed *Puraka* or filling in. It is followed generally by a pause which is directed to be increased gradually by practice. This withholding of breath for sometime was termed *Kumbhaka*. The word conveys retention of breath in the lungs or thorax. The inspiration and pause of retention are followed by the third phase, viz., expiration or breathing out. This phase was termed *Rechaka*.

Patanjal's classification of Pranayama. We can now place before our readers the translation of several sutras of the Patanjal, which inculcate the principles of the *Pranayama*. "When there is that attainment of the stability of the posture or *Asana*, the interference or cutting off of (normal) sequence of the phases of inspiration and the expiration should be attempted. It is called the *Pranayama*. This *Pranayama* is external or internal or suppressed or incarcerated in type and being varied with respect to locality, time and number, may be long or protracted and short or subtle. There is a fourth type where the *Pranayama* is associated with some thought on objects external or internal."[18] Instead of commenting on the sutras of Patanjal we propose to discuss the various types which will throw light on the implication of the sutras. It will appear that in the *fourth type of Pranayama there is the combination of regulation or restraint on the breath with concentration* on some object outside the body or some idea. When this type of *Pranayama* is practised, the covering which normally clogs the mind wears off, and the original inherent power of the *Chitta* of knowing every thing gains expression.[19] As we shall discuss the methods of concentration later on, we restrict our remarks to the physiological process of the *Pranayama* at this stage.

For the beginner the rule is not to try any constraint on breath by trying to hold it. Inspiration is followed by expiration, care being taken not to cause any discomfort. This is called *Nadi Suddhi*. It is to be practised for six months and it prepares one for later stages in which the *Kumbhaka* comes to play its part. The *Pranayama* at this stage now consists of *Puraka*, *Kumbhaka*, and *Rechaka*. The closing of the nostrils is effected through the

[18] Patanjal, *Yoga Sutras*, Chap. 2, Sec. 49-51.
[19] *Ibid.*, Chap. 11, Sec. 52.

help of the thumb, the middle and the ring fingers of the right hand. As we have noted above *Puraka* should be begun with the left nostril and corresponding *Rechaka* will end through the right. When the *Pranayama* is repeated without pause after the first for the second time, the inspiration should be through the right nostril and the *Pranayama* should end with expiration through the left. The third *Pranayama* should be a repetition of the first and the fourth a repetition of the second and so on. Now as to the time of these three phases there are two rules. According to the first, which is the vedic mode, the three phases should be of equal length or duration. The second or the *Tantrika* mode seems to be an improvement on it as a result of experience, the relative magnitude of the duration of these phases being 1:4:2. For the convenience of regulating time, the speechless chanting of certain short or long mantras is generally selected.

The directions are very emphatic on the point that the expiration should be effected very gradually and that no initial length of inspiration should be chosen or attempted which would entail discomfort in the process of *Kumbhaka*. The *Kumbhaka* and the *Rechaka* should be so effected that there should not be any panting for breath after a *Pranayama*.

There are two types of *Pranayama* according as the emphasis lies on the *Rechaka* or the *Kumbhaka*. In the former, efforts are made to squeeze out the air from the lungs and the body and to continue in that stage as long as possible. In the latter, the aim is to fill up the lungs with air and then to withhold the breath and to continue in that stage as long as possible. It is believed when the *Kumbhaka Pranayama* is satisfactorily acquired the air from the chest permeates through the whole body and may come out through the pores of the skin. The initial degree of success attained is marked with *perspiration coming out from the body*. Incidentally it may be noted that there is a direction for not wiping off the sweat but to rub it over the body for evaporation. The second stage of success is marked by *trembling*. Perfection is attained in the third stage; the body becomes *light enough to rise from the ground at will*. The incarcerated air under pressure is supposed to force its own way into interstices of the structures in the thorax and abdomen. The Hatha Yogis believe that the air within the body can in course of practice force open the mouth of the *Sushumna* or the spinal canal which usually remains plugged by *Kapha* or phlegm.

At this stage we have to refer to the Hindu physiological conception of *Vata* or *Vayu* which has been expounded before. The *Vayu* or air is marked by the dynamic or driving force, whether it is of gross form—the atmospheric air—or a nervous force or impulse in its subtle form. In fact it comprises the gross air and nerve force. Or we may put it in another way that movement results under nervous impulse where the wind plays its part in more or less attenuated form. By no stretch of imagination may we equate *Vayu* as a nervous impulse.

(5) We now pass on to the fifth method which is psychological and consists in directing the attention to a particular portion of the body, on an image, on any sense function or organ, or anything in space. This direction of attention may or may not be coupled with some form of imagination or idea. It is well known that when attention is directed to any physiological organ or part of the body there is some sort of physiological change. The function of the organ may be accelerated or quickened. In short we may state the dictum of the Hindu Yoga, that *what can be attained by the regulation and restraint of breath can also*

be effected by directing the attention to the particular localities and combining such fixation of attention with different types of imagination. Even the *Hatha Yogis* declared that the *Pranayama* process should ultimately lead to *Raja Yoga* through the *Kundalini Yoga* or forcing the air through the *Sushumna*. In *Hatha Yoga* works there are definite instructions to combine the meditation with the operations of breath regulation and the direction of air force in specific channels.

Principles of Raja Yoga. The cardinal principles of *Raja Yoga* may now be stated in a nutshell. When the mind or personality has been properly purified of impurities by various means, attempts should be made for reducing or steadying the *Vrittis* to a minimum by concentration and rejection of unwanted ideas creeping into consciousness. After the practice of concentration is acquired attempts should be directed not to think of anything. When this state is attained there is the experience of the true experiencer, *Purusha* or soul. This process is called *Manolaya* or annihilation of mind, from which results the acquirement of true knowledge. It may be pointed out that at some deep stage of concentration the number of the mental entities of contemplation is reduced to one. When we think of anything three items are involved: the object of experience, the experiencer, and the subjective content of experience or some specific idea or knowledge. It is the first two that are lost. This is the introspection of the state of contemplation. That the oscillation and fluctuation of the mind is very difficult to check or curb was noticed in the earliest times and the Yogis formulated very effective purely psychical methods which we shall discuss now. They are various effective methods in securing the *Samadhi* or concentration.

(a) The first is called *Pratyahara.* The Patanjal says, "The withdrawal of the *Indriyas* (inner sense organs) from their own objects leads to their attaining as it were the true nature of *Chitta*—the thinking principle. This is called *Pratyhara.*"[20] "After that or through that the Indriyas or sense organs come under complete subjugation."[21] It is the tendency of the inner sense organs to seek objects outside. When the external sense organs are physically obstructed the aim is not achieved for the images continue to develop and elaborate. This *Pratyahara* is a process of curbing the outward tendency of the sense organ.

There is a negative and a positive way of doing *Pratyahara.* When we attend to anything, other *Indriyas* are in a state of *Pratyahara.*

[20] *Op. cit.*, Chap. 2, Sec. 54. [21] *Ibid.*, Sec. 55.

This is the negative method. The positive method consists in turning out the intruding ideas after selecting a particular sense organ. Attention is restricted to the organ itself with a determination not to attend to objects that intrude on it. The example of attending to the sound after closing the two auditory meatus for some time each day for about two months is a very effective one for attaining *Pratyahara*.

(b) There is still another method, consisting of four parts: (*1*) The process of binding the thinking principle to any place localised in space or on the body or within the mind which is called the *Dharana* or fixation.[22] It is not enough. (*2*) When a particular idea or cognition is continued without a break it is called *Dhyana* or contemplation.[23] (*3*) When the same (contemplation) appears in content or meaning only as if devoid of its own nature (the *Vrittis* or fluctuation of the thinking principle) it is called the *Samadhi* or concentration.[24] In this state there is no knowledge of the experiencer or object, only the knowledge takes possession of the thinking principle. (*4*) When these three are together in one, *i.e.* practiced with respect to the same thing, it is technically called *Samyana* or constraint.[25]

The Patanjal states: "When the *Samyama* is conquered or acquired thoroughly there results what is called *Prajnaloka* or the light of knowledge or insight.[26] The principle of developing constraint may be looked upon as the greatest discovery of Indian experimental psychology. In fact as a point cannot be defined without three coördinates, so the three processes of fixation or location, imagination and repetition of the idea are necessary for the development of the state of constraint. This faculty of constraint, *Samyama*, is the most powerful tool or lever in hands of an ardent student or practioner of Yoga or psychology. What the Yogi can attain by applying the tool in different directions are to be found in Patanjal.[27] In fact it is believed that this psychological apparatus brings within the reach of a Yogi supernormal power and knowledge. The *Samyama* is to be practised slowly stage by stage beginning with the gross and ending in the subtle ideas. The attention of the reader should be drawn to several points in this connection.

(*1*) Normally two things are noticed with regard to fixation. The idea fluctuates in intensity or there may be absolute lulls. The other is oscillation from the particular idea to another. According to one statement, if the duration of attention be indicated by a moment,

[22] *Ibid.*, Chap. 3, Sec. 1.
[23] *Idem*, Sec. 2.
[24] *Idem*, Sec. 3.
[25] *Idem*, Sec. 4.
[26] *Idem*, Sec. 5.
[27] *Op. cit.*, Chap. 3.

fixation (*Dharana*) would mean the duration of 12 times that of attention. The duration of contemplation (*Dhyana*) would be 12 times that of fixation (*Dharana*), of concentration (*Samadhi*) 12 times that of contemplation (*Dhyana*), and of constraint (*Samyana*) 12 times that of concentration (*Samadhi*).

(2) Suitable objects of fixation ordinarily are the knot of nerves at the navel, the locus of the heart, the light in the head, the tips of the nose and tongue, etc. Perhaps the physiological importance of these centres led the Yogis to select these places. Of these, the heart seems to have been the subject of constraint from the very earliest times. The heart was regarded as one of the four seats of the soul, viz. navel, heart, throat and the head. The heart was regarded as the centre of feeling—affective experiences were referred to the heart. It is also declared that when constraint is applied to the heart there is experience of light and there is absolute cessation of distress due to separation from a loved object.[28] There results also a true knowledge regarding mind or Psychology if one practises constraint on the heart.[29]

(3) Very frequently a certain degree of latitude in the matter of contemplating the parts or limbs of the thing or idea is allowed. This relaxation of oscillation is helpful in developing the constraint. This principle of constraint is taken advantage of for religious purpose. Thus to contemplate on a particular deity the attention should be fixed to a part of the image. There should be a background—the image of the deity—the chanting of *Japa* of the mantras indicative of the deity, and lastly the circumspection of the different parts of the image in cyclic order.

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[28] Patanjal, *op. cit.*, Chap. 1, Sec. 36.

[29] *Ibid.*, Chap. 2, Sec. 35.

THE MAGICAL BEHAVIOR OF CHILDREN IN RELATION TO SCHOOL

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Several authors—among them, Piaget,[1] Werner,[2] and Zeininger[3]—have ascertained that children and older youths behave in a manner known as magical. According to Piaget, this magical behavior is based upon participation. Participation (a term created, as we know, by Lévy-Bruhl) is the relation which appears between individuals or phenomena which are considered to be partly identical or closely influencing each other, even when there is no spatial contact or any causal connection between them. Participation and magic should, says Piaget,[4] be distinguished from childish animism, *i.e.* the tendency to ascribe life and consciousness to inanimate objects. These phenomena, though they may be separated *in abstracto*, are actually closely associated. For instance, children believe that the sun follows them. When the children emphasize the activity of the sun itself, this is animism, but when they think that they themselves produce this phenomenon, it is magical thinking, based upon participation and containing an element of animism. Piaget states that animism grows out of participation.[5]

An interesting systematization of the magical behavior of children has been given us by Werner, whose term 'magical' includes also various activities in which children imagine that they are influencing nature or people by methods outside the sphere of technique and natural causality. A magical attitude appears very early in children, and shows every possible variation from the playful to the religious, whereas in older persons the magical attitude may manifest itself in a sharp opposition to 'natural' ways of behavior.

Literature possesses many works discussing magical thinking among

[1] Jean Piaget, *La représentation du monde chez l'enfant*, 1926, 1–424.
[2] Heinz Werner, *Einführung in die Entwicklungspsychologie*, 1926, 1–360.
[3] K. Zeininger, Magische Geisteshaltung im Kindesalter und ihre Bedeutung für die religiöse Entwicklung, *Zsch. f. angew. Psychol.*, Beihefte 47, 1929, 1–155.
[4] Piaget, *op. cit.* [5] Piaget, *ibid.*

civilized peoples, members of primitive races, and the mentally abnormal, as well as special psychical states, such as fatigue, day-dreaming, etc. These problems, raised by such authors as Bartlett,[6] Lévy-Bruhl,[7] Mayer-Gross,[8] Storch,[9] and Abel,[10] have formed the object of many controversies; even now, the theoretic situation does not seem to be definitely cleared up. Here we leave aside such problems as whether primitive mentality differs from civilized mentality, whether the childish mind shows a similarity to the primitive, whether in every mind exist archaic layers that manifest themselves in given conditions, and whether these layers are a fiction springing from an insufficient consideration of social influences.

The magical attitude of children grows, in Werner's opinion, out of certain characteristics of the child mind, from the interpenetration of experiences and activities, and because of the fact that the childish *ego* is not clearly distinguished from the world surrounding it and is more vividly affected by it. On this psychic basis arise certain habits, which pass into magical ceremonies, if the discontinuance of the ceremony is felt as causing some harm or destruction, or its performance as productive of pleasant consequences. In the list of magical ceremonies belong, *e.g.* some childish habits before going to sleep—found even in three-year old children—like holding a handkerchief or ribbon in the hand, reciting a dialogue which always ends with the same words, etc. From this magical attitude of children may develop a feeling of the predestined association of the self with nature, which becomes the basis for a conviction that the world may be controlled by magical methods of acting. Most frequently these magical methods are combined with natural technical ways of acting, as in the example of a ten-year-old girl who does her school-work conscientiously, realizing that learning is a natural condition for obtaining good grades, but nevertheless believes that she will receive a good grade only if she writes with a special, lucky pen-holder. This child understands the natural connection between work and her school grade, but outside of this there exists for her a magical connection between the lucky pen-holder and school grades.

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[6] F. C. Bartlett, The psychology of the lower races, *VIII. Intr. Cong. Psychol.*, 1927, 198–202.

[7] Lucien Lévy-Bruhl, *La mentalité primitive*, 1922, 1–538.

[8] Willy Mayer-Gross, Primäre und sekundäre Symptome in der Schizophrenie, *Zsch. f. d. ges. Neur. u. Psychiat.*, 124, 1930, 647–672.

[9] Alfred Storch, *Das archaisch-primitive Erleben und Denken der Schizophrenen*, 1922, 1–89.

[10] T. M. Abel, Primitive and child mentality: A discussion of their significance, *Kwart. Psychol.*, 5, 1934, 273–289.

From this example, we see that magical thinking may refer to matters associated with school life and the school. The school as an institution making definite demands on the youth in a more exacting way than the parental home, by examining and classifying pupils, often forms in a young person the foundation for anxiety and fear, hope and doubt, whether the pupil can fulfil the demands made upon him. In such cases, many pupils take recourse to magical methods of influencing the course of school affairs. In the interesting work by Zeininger may be found various instances of the magical behavior of children in situations connected with the school. The present work aims to analyze systematically the 'school' magical behavior of girl pupils in three gymnasium classes and of some university students.

Girls of Third Class

Let us look first at the magical behavior of pupils in the third class of a girls' gymnasium in Lodz.[11] These pupils, for the most part between 12 and 13 yr. of age, stand on the lowest age-level on which one can use successfully the method of collective questionaries: with younger pupils one must resort to individual investigations by means of Piaget's 'clinical' method.

Thus, in this class (composed of 39 girls), 8 girls (about 20%) do not show any evidences of magical behavior, 18 girls (about 46%) confess to school magical thinking. There are 12 girls (about 30%) who do not acknowledge any magical phenomena in relation to school, and one girl who does not believe in magical practices, writing that "they seem to me unwise."[12]

To illustrate the magical behavior of this class, I may cite a few examples, in the endeavor to show all the types of school behavior which belong to this class.

[*1*]:[13] "I always walk on the side of the street on which the school is situated, since I believe that if I walk on the opposite side, I will get a 2 ('insufficient')."

[*2*]: "When I wanted to get a good grade, I thought all the time about a bad one." "I consider the number 13 unlucky. Therefore, when a class exercise falls on the 13th., I ask to have it postponed until the following day."

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[11] By means of questionaries, much material concerning classes from 3 to 8 was collected, at my request, by Dr. Justyna Lawicka. My present work is based on the answers given by the pupils of the third class and those of the eighth, the highest class, after finishing which pupils pass a maturity examination and become eligible to enter a university.

[12] If we count this girl who does not believe in magical practices with the group that does not show any evidencies of magical behavior, the latter group is increased to 9 persons, about 24% of the whole number.

[13] The italic numbers correspond to the place which the girl giving the answer occupies in the roster of the class.

[*5*]: "On the way home from school, I raced a street-car to learn whether I would write a good composition or solve my problems correctly. If the car caught up to me before I reached the goal set, I knew that my ambition would not be realized."

[*11*]: "Before written tests, I don't eat sweets for two days."

[*14*]: "I have a talisman which makes me play ball well and not quarrel with my schoolmates. This talisman is the string of pearls I received from my friend on my name-day. When I don't wear it, I play ball badly."

Quite striking is the fact that in the magical thinking of girls of this class, a large rôle is played by the street on which the school is situated, as we see from the instance of Girl *1*, who states that she always walks on the school side of the street, since if she did not she would get a "2" (insufficient). Girls *5* and *6* assert the same. On the other hand, another girl (Girl *13*) says that her usual custom is to walk on the opposite side of the street on her way to school; for she believes that when she does not do so, she will get a "2." Here belongs also Girl *15*, who states: "When I go to school, I cross to the other side before the post, so as not to get a "2." With some changes this same motif of the street occurs also in the cases of Girls *2*, *8*, and *12*. The variation introduced by Girl *9* is the following: "When walking along the street, I take care not to step on the edges of the side-walk, because I want to succeed with my lessons."

From this comparison it is evident that the magical thinking of the girls in this class occurs most often on the street, when they are going to school. At such times—on the way to school—the mind is occupied with thoughts of what will happen in school. Fears arise as to success in class. There appears a desire to direct one's fate or to foretell one's destiny during the school hours about to begin. In this situation the mind, disturbed by the near future, seizes upon the material at hand and performs its magical practices on the street; the child goes along it in a particular way, crossing to the other side, passing an advertising board or a street-lantern, etc. In this manner, original forms of magical behavior may arise spontaneously; while others, learned from schoolmates, may be applied imitatively.

Girls of Eighth Class

Lodz gymnasium. Let us turn now to the eighth class of the same gymnasium to which belong 17 to 18-yr.-old girls. The material at my disposal refers to 17 such girls. All these girls know magical thinking from their own experiences; but 7 of them (*i.e.* 41%) have ceased to use magical practices, while 9 (*i.e.* 53%) affirm that these practices are dying out of them, and that they resort to them on important

occasions only, "sometimes," "infrequently," etc. Only Girl *5* confesses that she practices all these habits "frequently, even now." In reference to school, this very pupil testifies: "I have the habit, on the way to school, of crossing in front of a particular tramway-stop, in order to avoid getting a '2'." In general, only 6 pupils of this class (*i.e.* 35% of the whole number) show school magical phenomena.

Among the 9 girls whose magical behavior has already ceased are some who had a very rich magical experience, which now, however, becomes active only before important events. For instance, Girl *1* writes:

> Before an examination in school, I counted the pictures in the room. If there was an even number of them, I said I would pass the examination; but if an uneven number, that meant that I would fail. Before taking an examination, I always said: 'Today I shall surely fail,' but I thought that I really would succeed.

In this class also are found those magical practices, employed in connection with school, which consist in counting objects (*e.g.* pictures) in order to discover whether their number is even or uneven, or performing some activity a definite number of times (*e.g.* stepping on three successive blocks of the sidewalk), etc. Such phenomena, in which the counting of objects or the analysis of figures enters into magical practices (but also occurs outside of them), I call "arithmothymic,"[14] and here I touch them only incidentally.

The pupils of the eighth class who discussed the genesis of such phenomena ascribe them to anxiety in the face of important events (if in school, examinations and written tests). "These habits," states one girl, "spring from anxiety before important undertakings." She concealed them "for fear some one might think them silly and childish." Another girl declares: "These habits spring from fear of failure in some undertaking, and sometimes even from a general fear of failure."

Busemann once remarked pointedly that youth is more conscious of the school than of the parental home, but the influence of the parental home on the youthful mind is more effective.[15] The demands which the parental home makes on children are completely comprehensible to them, whereas those of the school seem stranger to them. The school grades every activity of the child, praises or blames him in front of the whole class, and gives him many surprises in connection with oral examinations and written tests, whose themes are unknown. There-

[14] Stefan Blachowski, Arithmothymic inclinations in youth, *IX. Inter. Cong. Psychol.*, 1929, 81–83.

[15] A. H. H. Busemann, *Die Jugend im eigenen Urteil*, 1926, 1–252.

fore, it arouses fear by the unknown quantity which the pupil encounters in school. In the light of this explanation, quite understandable becomes one of the results of our investigation; namely, that very rarely do we find in the pupils' answers any magical practices referring to the parental home, though we discover very many such practices referring to the school. Fear of failure in school is at the same time that fear of the unknown which we have just mentioned. A penetrating analysis of the unknown and of fear of it, as well as of their mutual relation, has been made by Oscar Liebeck. This author demonstrates that fear is a correlate of the unknown. In a fear situation, a person becomes helpless, uncertain, and disturbed. He cannot find the proper means to control the unknown. Therefore, he often in his helplessness grasps at senseless and aimless methods of acting,[16] like the various magical practices which are performed to insure success, but which are entirely unsuitable for that purpose. Werner has already called attention to the fact that magical practices always increase at periods that are uncertain or dangerous for individuals—as in school during examination time. On the basis of our investigations we may add that there exists a group of school magical practices that are performed before entering the school building. Many pupils make use of magic, as we have seen, on the way to school; and an especially significant moment is entrance into the class-room. From the time the pupil enters the class-room and becomes incorporated into school life, active magical practices cease, and only passive practices act through the magical forces previously prepared—like old uniforms (specially put on for the occasion), amulets, lucky pen-holders, etc.

Katowice gymnasium. Particularly interesting appears the questionary-material concerning the 8th class of one of the girls' gymnasiums in Katowice.[17] The pupils of this class, 23 in number, answered the questionary after their written maturity examinations and 3 days before their oral examinations. There were, in this class, only 2 pupils unacquainted with magical thinking from their own experience, and only 3 who had formerly used magical practices, but had given them up—even during the examination period. The others, *i.e.* 18 persons (or

[16] Liebeck writes: "Die Angst, allgemein charakterisiert, bedeutet ein Streben, das wegtendiert von Unbekanntheit. In ihren stärkeren Graden kann sie lähmend wirken: sie erstarrt. Es ist charackteristisch für die Angst, dass die Handlungen, die sie auslöst, meist sinnlos und unzweckmässig sind. Dies rührt von der Unsicherheit her, in der sich das Lebewesen befindet, wenn ihm etwas Unbekanntes begegnet" (O. Liebeck, *Das Unbekannte und die Angst*, 1928, 83).

[17] I am indebted for this material to the kindness of Dr. Tadeusz Dobrowolski, the class teacher.

78%), give evidences of magical thinking. In 7 pupils of this group, magical thinking increased during the maturity examinations; with 6 it remained on the same level as formerly; but in 5 others, it was weaker. Of course, the degree to which this tendency is strengthened or weakened is not identical in all these persons; but this we cannot discuss further because of lack of space.

In comparison with the class mentioned above the girls of the class in Katowice show much greater tendencies toward magic. Whereas in the former, 41% did not use any magical practices, here the percentage of those who never used it or have ceased to use it amounts to about 22% only. Whereas in the former class, 53% of the girls testified that their magical practices are dying out, here the relative proportion is only 30%. Finally, in the class in Katowice we discovered an increase in magical phenomena in 7 pupils (*i.e.* 30%), whereas in the Lodz class only one girl acknowledged that she resorted to magical practices "very often" at present. In short, the Katowice class is obviously permeated much more strongly by magical elements than the Lodz class, in which the liberation from irrational ways of behaving has progressed considerably further.

From the Katowice material we may deduce that certain magical practices are accepted by the majority or, at any rate, by a large proportion of the pupils, becoming in this way "class" magical practices. Some pupils understand their mutual influence and the almost general acceptance of certain practices, as is proved by the following statements.

[*B*]:[18] "No girl in our class (myself included) will ever sit in the place of a girl who has been debarred from the maturity examinations."

[*N*]: "The superstition is current among us, that every third pupil to enter the class-room will get a bad grade that day. Besides this, if we ever meet a chimney-sweep, we grab hold of one of his buttons, so as to have some good luck. These beliefs have, of course, so thoroughly permeated our class, that even if I didn't believe in them, I would involuntarily pay attention to them—all the more so, because it often happens that our superstitions get confirmed."

[*T*]: "The belief is common among us that every third person to enter the class-room will be unlucky."

Of such magical practices, the most frequently mentioned in the answers to the questionaries is, "not to be the third to enter the class-room." Twelve girls give it as their habit, which suggests the possibility and even the probability that other pupils also make use of this magical means of avoiding bad grades, but forgot to mention it.

[18] The individual pupils of this class are designated by italic letters.

Several pupils also state that one should never walk with classmates according to height, because afterwards there is always some trouble in school; and that old uniforms bring good luck at maturity examinations, new ones bad luck. Thus, in this class we encounter prohibitions as well as positive directions; Do not be the third to enter the classroom; Do not walk according to height. Breaking these prohibitions brings bad luck.

We discovered a considerable difference in the magical thinking of these two classes, though they are on the same level in the school system, follow the same curriculum, and are attended by pupils of the same age. To explain the reasons for this difference is, however, a difficult matter. It may be due to social factors, if the two classes are recruited from different social groups which vary considerably with respect to the intensity of their magical attitudes; or to educational and pedagogical factors, if the teachers of one class try harder than the teachers of the other class to induce the youth to think clearly and accurately, penetrating to the actual causes of the phenomena they observe and in this way developing scientific behavior. It is most probable, however, that in the Katowice class are to be found individuals who, themselves showing magical phenomena to an unusual degree, have influenced suggestively the others, infecting psychically almost the entire class. That this hypothesis is correct is proved by three bits of evidence. First, in the Katowice class there are actually girls who use magical practices in an excessive manner. Thus, Girl *H* says that during the period of maturity examinations her magical thinking "grows to unheard of dimensions . . . at every step, even." Secondly, some practices are identical for the greater number of pupils. Thirdly, many pupils explicitly declare that the example of their classmates led them into magical practices. Of the 21 persons in the 8th class who use or formerly used magical practices, 14 (66%) expressly declare that they were influenced by the example of their classmates (sometimes, besides this, by their home environment or other factors). Three persons (Girls *M*, *P*, and *R*) affirm generally that their magical practices are due to "the example of others," "partly to the environment," and "to my own experience." Finally, one (Girl *T*) says that some of her practices she heard about, others she developed herself.[19]

[19] Girl *Z* states, concerning her magical practices, that "they all developed spontaneously." Girl *Y* says that "the example of class-mates or the encouragement of others has not played any rôle, since it (magical thinking) arose spontaneously as a consequence of the urgent desire to have wishes fulfilled." Finally, Girl *I* declares that, aside from the

Looking over the answers of Girls *M*, *P*, *R*, and *T*, we notice that *R* and *T* adopt precisely those practices which are most common in the class (not to be the third to enter the class-room and not to walk to school according to height). From this it results unequivocally that the two pupils who say that their magical practices are due "to the example of others" had in mind the example of their classmates, though they did not say so explicitly. In view of this, the group of 14 pupils influenced by their classmates is increased to 16 (or about 80%). We can, therefore, consider it settled that the magical thinking of pupils in relation to school arises in the great majority of cases under the influence of classmates who perform magical school activities.[20]

University Students

As to university students, I have materials collected by questionaries in the years 1931 and 1937. The investigation of 1931 refers to 124 persons (94 women and 30 men); that of 1937, to 49 persons (28 women and 21 men). In the present work I shall consider only the answers given in 1937. Members of the psychological seminar took part in this investigation, being already acquainted with the problem of magical thinking. In this group, there were 10 persons (6 women and 4 men), or about 20% of the number investigated, who testified that they were unacquainted with magical phenomena from their own experience. However, 39 persons (22 women and 17 men), or about 80%, confessed that at present or formerly magical phenomena occurred in their experience in varying degrees of intensity. Let us look more closely at these phenomena, beginning with the group of women.

Women students. Of the women, 28 in number, 6 (21%) are unacquainted with magical thinking—or so they said; 4 (14%) have completely ceased to use it; 10 (36%) are using a decreasing amount of it; 7 (25%) use it as formerly; and only 1 (4%) in an increased degree. Taking the first three groups together, we may state that for 20 women (71%) of university rank, magical thinking either does not exist at all or is dying out.

Of the 18 women students (65%) who during their university studies still show magical behavior (independently of the question whether

influence of example, in spite of herself she began to believe partially in the effectiveness of magical thinking.

[20] Girls *M* and *P*, it is true, also state generally that they were influenced by example. From their answers it is, however, impossible to decide whether they imitated classmates or other persons of their environment.

it has decreased, increased, or remained the same), not all apply magical practices in academic life. Some of them do not use these practices because (so they say) they have no occasion to do so: they have not yet been studying a whole year in the university (women students *U-4*, *U-8*, and *U-14*). Of these, two explain that they have not yet taken any examinations. For six others, magical practices are connected with other domains of life, not with academic work.

Nine women students, on the other hand, apply magical practices also in academic life. Here are their statements.

[*U-2*][21] "I use magical thinking before examinations. I think strongly, so as to be asked from a given chapter."

[*U-3*]: "It (magical thinking) appears in my academic life. For instance, I sometimes wish the lecturer to look at me. If he does, I believe that I will obtain something about which I thought at a certain time."

[*U-6*]: "During my studies, faith in various prejudices has become much weaker. I do not attach an importance to the places or the persons with whom I come into contact in my university work. Sometimes, I still believe in the good luck which amulets bring me, especially during examinations. I doubt whether I should have the courage to go to an examination without my little chimney-sweep. But I don't exaggerate the matter or believe that he must absolutely bring me good luck."

[*U-7*]: "During my studies. If I am ever convinced that I am prepared for something, *e.g.* an examination, it proves later that I know nothing. On the contrary, sometimes I study, but it comes very hard to me or I am unable to repeat it independently: my head seems quite empty. But after a while, I answer well the questions asked me concerning the material I have studied. Before an oral quiz, I always dream something and try to deduce from it a good or bad result."

[*U-12*]: "In my academic life, these magical practices (belief in dreams, talismans, etc.) have weakened; only during examinations they grow stronger."

[*U-15*]: "This year, when going to an oral examination, I turned back, just to put on a bracelet I had forgotten to wear. It seemed to me, and I even believed, that I would feel better with it."

[*U-20*]: "During the Teachers Course examinations I tried to think as little as possible about the problems I didn't understand, because I believed that then there would not be any questions asked about them, but perhaps this crosses over into the sphere of suggestion."

[*U-13*]: "In reference to examinations, etc. my magical thinking has weakened. It has rather turned toward people."

[*U-22*]: "During my university period, magical beliefs weakened. They refer chiefly to dreams. During examinations I don't ascribe much significance to them, though I believe that certain flowers taken to an examination bring good luck (*e.g.* pansies)."

Men students. Let us now turn to a group of men, composed of 21 persons. In this group, there are 4 (19%) who are entirely unacquainted with magical phenomena from their own internal experience; and 17

[21] The numbers in italics following "*U*" refer to individual women students.

(81%) who experience or have experienced magical thinking in various ways and in varying intensity. In the latter group, only 5 men told about their magical practices in relation to university life; 2 did not answer at all on this theme; 4 have given up entirely the magical practices which they used in the middle school; 5 use magical thinking, but not in connection with university matters; and finally, one person testifies that he has never used magical thinking in reference to university matters.

Analogically to the investigation of the women students' group, we determine that among the 21 men students, 4 (19%) have never experienced magical phenomena; 6 (29%) have ceased to use magical practices at the time of the investigation; and for 5 (23%) magical tendencies were decreasing; while 6 (29%) remained more or less on the same level as formerly. Thus, 11 students (52%) still show magical phenomena at the time of university studies. But it further appears that, at this time, for 15 students (71%) magical thinking does not exist at all or to a diminishing degree. The group of men students when compared with the group of women students proves very similar, since in both groups the proportion of persons for whom magical thinking does not exist at all or to a diminishing degree amounts to 71%.

As we have already mentioned, in the group of men students only 5 spoke about their magical thinking in connection with university life. The following were their answers:

[*U-G*]:[22] "Even on the university benches, I often pass through magical thinking, perhaps unconsciously, and that in connection with examinations . . . I give one clear instance. Sometimes I think that when the hour strikes for standing in the presence of my professor, I will observe whether he enters the room with his left or his right foot. The left means success for me. Or if the professor is the first person I meet on the university grounds, this means failure to me."

[*U-H*]: "I believe, *e.g.* that among the days of the week, Thursdays, but especially Saturdays, are particularly fortunate for me. If an examination falls on these days, I feel better and am more certain that I shall pass. Especially here (at the university) I have passed easily the tests taken on Saturdays and Thursdays."

[*U-L*]: "A friendly look from a sympathetic woman student brings as an immediate result failure at blackboard exercises or in my studies in general. Whenever I am loath to go to a lecture, usually I would lose much by not going, for then the lecture is sure to be especially interesting. And, vice versa, when I go enthusiastically to a lecture or an exercise, I come out disillusioned, *i.e.* I learned little that was new or found the lecture boresome. If I am afraid of an examination, I know subconsciously that all will be well. But if I feel sure of myself, something gets muddled up."

[22] The italic letters following "*U*" refer to the individual men students.

[*U-M*]: "Before an examination magical thinking comes out strongly in me. In relation to professors, I imagine that, if I feel even a covert antipathy to any one of them, I will have a hard time getting through his examination."

[*U-N*]: "I don't connect any magical thinking with examination periods, so long as they have nothing to do with the number 14, but if the examination falls on the 14th of the month, or takes place in room 14 or anything like that, I am prepared for the worst."

The magical behavior of university youth presents a different picture from that of youth in a gymnasium. It not only appears more rarely, but generally has another character, since the magical practices are much less permeated by attitudes that can be called magical. When, for instance, we read that *U-7* learns with difficulty and has an empty head, but afterwards answers well, or that *U-1*, when he goes to a lecture enthusiastically, comes out disillusioned (or vice versa)—here we are dealing with elements which can be explained by a rational psychology, and which at bottom diverge greatly from real magical phenomena.

Among the youth in middle schools magical practices lead most frequently to very simple activities, which are performed in an identical manner by a smaller or larger group of pupils in a given class. Some of these practices, as we have already emphasized, may even be considered simply as class practices, since they are recognized by the whole class or are, at any rate, known to the whole class. Among the university youth, such magical behavior cannot be discovered. Here magical practices are purely individual: the influence of classmates is at a minimum. This may be explained by the different environment in the university, where greater freedom prevails in the choice of lectures and exercises, so that there are no large closed groups like school classes. Moreover, in the Polish universities, there is no compulsion to pass examinations continually, nor, as compared with the middle school, continuous home-work or class-tests, with grades. As we saw in discussing magical thinking in middle schools, it is precisely these demands, made by the school and expressing themselves in the grading of pupils for home and school work, behavior in school, oral and written examinations, etc., that cause the most various magical phenomena. The university does not make demands of the same kind as the middle school; hence the great difference between the magical behavior of school youth and that of university youth, about which we spoke above. In one respect, however, we find much similarity in the youth of the two types of school, in that the time when magical school thinking is most strongly liberated is at examinations.

The following table presents a general review of the frequency with which magical phenomena appear in the given classes and in the university. The first column shows the percentage of those who confess to magical thinking at any time whatsoever, and the second column the percentage with school magical thinking:

3rd class of Lodz gymnasium	76	46
8th class of Lodz gymnasium	59	35
8th class of Katowice gymnasium	78	70
Women students at the university	65	32
Men students at the university	52	24

We notice that the percentage of those who, according to their own account, show magical behavior in relation to school, very obviously decreases as they reach the upper school levels. The only exception is the 8th gymnasium class in Katowice, where the percentage of pupils using school magical practices jumps violently and becomes the highest in the whole column of figures. We know from the above discussion that the cause of this is the special state of uncertainty and strain that accompanies the period of difficult maturity examinations.

Discussion

In his work on psychology and religion—worth the attention of others as well as of psychologists—Forsyth, to explain the relation between science, religion, and magic, goes back to Freud's conception of two ways of thinking: pleasure thinking and reality thinking.[23] The former manifests itself especially in dreams and day-dreams; the second is concerned "with adapting our individual wants to the conditions of our environment—the kind of thinking, for example, which has to be used in all vocational work such as we undertake for our bread-and-butter." Pleasure thinking, dominant in childhood, when the child realizes his desires in imagination, later becomes more and more overbalanced by the reality thinking which develops under the pressure of the hard conditions of life and objective acquaintance with the reality of everyday practice, particularly under the influence of rational education and the natural sciences. In the course of years and in suitable conditions, the individual gains an understanding of the factors on which depend the results he wishes to obtain, and recognizes that dreaming about a certain result, even with the aid of some magical activity, does not yet guarantee success. As regards school magical

[23] David Forsyth, *Psychology and Religion*, 1935, 1–221.

thinking, it may be said that the pupil who desires to get a good grade anticipates in his day-dreams his success in school and rejoices at the thought of the good result. He certainly knows that the natural method of winning success is honest study, which is not always associated with pleasure; but he believes in addition—the depth of his belief varying greatly—in the effectiveness of his magical practices. Studying—so argues the magically thinking pupil—is one of the conditions of school success, but a condition that is sometimes insufficient (as proved by school experiences). Subjective security is given only by magical activity, with which is connected an immediate feeling of pleasure by the imagination of future, but already insured, success. We have seen that with the pupils whose magical thinking has ceased or weakened and whose reality thinking has triumphed, their magical tendencies sometimes revive or increase in situations that are difficult, uncertain, or terrifying.

We may, therefore, on the basis of our questionary-materials risk the statement that the later in life one's magical thinking ceases, the easier it reappears in periods of anxiety or fear. It should be emphasized, finally, that the natural way of getting a good grade in school is difficult, whereas the magical way does not demand any effort—certainly one reason for employing magical practices.

Zeininger distinguishes 3 types according to the attitude they show toward magical phenomena, and states that these types are already evident at the beginning of the period of naïve realism.[24] The magical type ("magisch gerechteter Typ") manifests already in childhood a magical behavior, as in games in which an element of animism and intuition dominates. In the intellectual type ("straff intellektueller Typ") is seen, even quite early, a tendency to seek the truth, to uncover real connections and causes. Children of this type are, during the realistic period, interested in technical games; whereas the play of children of the magical type has during this period a strong illusionary character. Finally, the type dependent on authority ("authoritär gebundener Typ") is characterized by the fact that in its thinking and acting it is only a reflection of what its environment is thinking and feeling. Persons belonging to this type are superstitious in a superstitious milieu, but are freed from their superstitions in a rationalistic milieu. Among the persons we invesgated were found all three types; some created independently their own school magical practices; others never turn to magical practices, even in the most uncertain school situa-

[24] Zeininger, *op. cit.*

tion, but always direct their conduct according to rational principles; and finally, others are influenced by the example of their comrades, readily accepting magical ways of behaving in school. Thus, the school milieu is a microcosm in which the same magical and rationalistic tendencies are at work as in the macrocosm of human society.

ZUR KLÄRUNG EINER TATSACHENFRAGE IM GEBIETE DES PSYCHOGALVANISCHEN PHÄNOMENS

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(Founded in 1932)

Als der Schreiber dieser Zeilen im April 1929 am XI. Kongress für experimentelle Psychologie in Wien über die regelmässige Übereinstimmung zwischen Gefühlsqualität und Ausschlagsrichtung des Galvanometers berichtete, gründeten sich seine Ausführungen auf einwandfreien Beobachtungen, die er seit mehreren Jahren verfolgte. Im genannten Vortrage hatte er als ersten reinen Fall das Ergebnis einer Demonstration des psychogalvanischen (p.–g.). Phänomens angeführt,[1] wo ganz unerwartet ein Student eine Reihe von Reizwörten mit p.–g. Reaktionen verschiedener Richtung beanwortete, wobei sich herausstellte, dass die eine Richtung mit Lust, die andere mit Unlust koinzidierte.[2] Diese Erscheinung wurde, wie damals hervorgehoben, durch Versuche an ungefähr 40 Vpn. überprüft und bestätigt. Die Methodik ist dort kurz angegeben, enthält aber alle wesentlichen Daten, die eine Nachprüfung der konstatierten Erscheinung durch andere Experimentatoren ermöglichen.[3] So viel uns bekannt, fand keine solche Kontrolle bisher ausserhalb unseres Laboratoriums statt, obwohl von mancher Seite die Wichtigkeit der in Frage stehenden Erscheinung, unter Voraussetzung einer Bestätigung, hervorgehoben wurde. Da uns begreiflicherweise am meisten daran liegt unsere tatsächliche oder vermeintliche Entdeckung von Anderen kontrolliert zu sehen und wir zudem glauben, dass die Sache selbst genügend wissenschaftliches Interesse beansprucht, möchten wir hier erstens eine ausführlichere Anleitung zur Wiederholung jener Versuche geben und zweitens auf anderweitige bestätigende Ergebnisse hinweisen.

Das anzuwendende psychologische Verfahren besteht darin, im

[1] Die psychischen Bedingungen der psychogalvanischen Phänomene, *Bericht ü. d. XI. Kong. f. exper. Psychol.*, in Wien, 1930.
[2] Es geschah dies während einer Vorlesung, in Gegenwart der übrigen Hörer, am 9. Dez. 1927.
[3] Von Carney Landis ist der Inhalt des Vortrags unrichtig dargestellt (cf. Electrical phenomena of the skin, *Psychol. Bull.*, 29, 1932, 717 f.).

Anschluss an ein Reizwort ein Gefühl hervorzurufen, wie es bei gewissen Assoziationsversuchen üblich ist. Die Vp. hat darauf keine verbale Antwort zu geben, sondern hat sich laut Instruktion in die Bedeutung des Wortes zu versenken. Hierdurch wird ein intellektuelles Erlebnis mit assoziativen Bestandteilen provoziert, welche die Grundlage für ein Gefühl abgeben. Wichtig ist dabei, dass der Versuch nur bei vollkommener Ruhe der Vp. vorgenommen werde. Man muss sich also dazu Zeit lassen; keinesfalls darf man eine Reihe von Versuchen in einem im Voraus bestimmten Tempo abwickeln wollen. Die Vp. soll möglichst an die Laboratoriumsumgebung gewöhnt sein und sich in guter Stimmung befinden. Diese subjektiven Bedingungen müssen erfüllt sein, da sonst keine Lustgefühle zu erwarten sind: die Vp. darf sich nicht in der Verfassung eines Patienten befinden, der zum ersten Mal beim Zahnarzt sitzt und bei jeder Bewegung des Arztes einen schmerzhaften Eingriff befürchtet! Um bei der Darbietung des Reizwortes gleichmässig konzentrierte Aufmerksamkeit vorzufinden, ist es unbedingt notwendig, den kommenden Reiz durch ein Signal, "Achtung," anzuzeigen. Die Darbietung kann akustisch oder optisch erfolgen. Nach der p.-g. Reaktion—wo eine solche ausbleibt, nach etwa 10 Sekunden—gibt die Vp. auf Aufforderung eine Aussage über ihr Erlebnis. Die Frage des Versuchsleiters habe eine möglichst stereotype Form und darf keine "Suggestivfrage" sein; wir fragen gewöhnlich: "Was haben Sie jetzt erlebt?" oder "Was haben Sie gedacht?". Aus der Beantwortung der letzteren Frage ist es leicht auf den Gefühlston zu schliessen. Die Aussage der Vp. dient als Hilfsmittel zur Interpretation der Galvanometerschwankungen. Sie ist aber nicht unbedingt dazu nötig, wie die weiter unten zu erwähnenden Tierversuche zeigen. Nach Beendigung jedes einzelnen Versuches hat man auf vollständige Beruhigung zu warten, bevor man mit den Versuchen fortfährt. Die eingetretene Ruhe ist aus dem Stillstand des Lichtzeichens an der Skala des Galvanometers zu ersehen. Hiermit kommen wir auf die Erörterung der technischen Anordnung, die zwar dem Kundigen nichts prinzipiell Neues bietet, die aber dem allfälligen Vorwurf ungenügender Angaben über die benutzten technischen Mittel begegnen soll.

Die Anordnung, die allein geeignet ist, unter bestimmten Bedingungen Ausschläge verschiedener Richtung zu geben, ist die Tarchanoffsche Anordnung ohne äussere Stromquelle. Das Galvanometer muss genügend empfindlich sein und darf nicht zu träge sein. Am besten ist ein Saitengalvanometer oder ein Oszillograph, wo sie aber nicht vorhanden, kann man auch ohne sie für unsere Zwecke auskommen.

Wir arbeiteten mit Drehspul-Spiegelgalvanometern nach Déprez-d'Arsonval. Zuerst mit einem Instrument, welches die Stromempfindlichkeit von 43 10^{10} A. für 1 mm. Schwankung bei 1 m. Skalenabstand hatte, später benutzen wir ein gleichartiges Galvanometer von 65 10^{10} A. Empfindlichkeit für 1 mm./m. Beide Instrumente haben ungefähr 100 Ohm Spulenwiderstand und sind aperiodisch.[4] Die Vp. stand mit dem Galvanometer durch Elektroden in Verbindung, die den Körperstrom von beiden Händen ableiteten. Als Elektroden wurden bei unseren Versuchen sowohl starre Elektroden aus Nickel oder Zink als auch unpolarisierbare Elektroden verschiedener Zusammensetzung benutzt, gewöhnlich Zink in Zinksulphatlösung. Die starren Elektroden wurden meistens zu Demonstrationszwecken verwendet. Bei den flüssigen Elektroden tauchte entweder die Hand oder der Finger direkt in die Elektrodenflüssigkeit oder stand mit ihr mittels physiologischer Kochsalzlösung in Verbindung. Es muss hier bemerkt werden, dass einwandfreie Elektroden, frei von Fehlern des Eigenpotentials und der Polarisation, für physikalisch genaue Messungen unerlässliche Voraussetzug bilden, dass jedoch solche Elektroden für die blosse Darstellung der richtungsverschiedenen Ausschläge bei Lust und Unlust nicht Bedingung sind. Allerdings ist darauf zu achten, dass die Potentialdifferenz der Elektroden nicht zu gross sei, auch sind Polarisationen, sowohl der Elektroden als des Organismus, zu vermeiden. Am einfachsten ist es die genannten Fehlerquellen zugleich mit dem Ruhestrom in üblicher Weise zu kompensieren. Wir benutzen dazu einen nach Thouless von Dr. Richard Hauptfeld in Zagreb konstruierten Apparat, der alle nötigen Änderungen im Stromkreis gestattet. Bei stromlosem Galvanometer kommen die erwarteten Erscheinungen am reinsten zum Ausdruck, vorausgesetzt, dass sich die Vp. in geeigneter Verfassung befindet und der Vl. mit der nötigen Umsicht verfährt.

Zusammenfassend kann man sagen: weder das psychologische Verfahren noch die technische Anordnung bieten besondere Schwierigkeiten. Beim Verfahren muss man bedenken, dass man Gefühle nicht durch Reizwörter zwangsmässig erzeugen kann wie Empfindungen durch Reize. Gelingt es aber auf indirekte Weise ein Gefühl hervorzurufen, so kann man seine Qualität durch eine relativ einfache Apparatur sichtbar machen. Die Sache ist also leicht zu bewerkstelligen, und ich hoffe daher, es werde sich nach den obigen Angaben jemand finden, der den Willen aufbringt, auf Grund eigener Beobachtung zu einem

[4] Hergestellt vom Hartman & Braun, Frankfurt, a. M.

Urteil in dieser Sache zu kommen. Wie schon im angeführten Kongressberichte bemerkt wurde, sind jedoch nicht alle Vpn. zu diesen Versuchen gleich geeignet. Man erwarte daher nicht, bei einer schlecht vorbereiteten Probe mit irgend einer Vp. sofort die besprochenen Erscheinungen zu erhalten. Wäre dies so leicht, hätte man die fraglichen Tatsachen schon längst festgestellt.[5] Bei Befolgung der angegebenen Methodik wird man sicher zu p.–g. Reaktionen gegensinniger Richtung kommen. Es bleibt dann nur noch die etwas schwierigere Aufgabe, diese Ausschläge auch richtig zu interpretieren. Dazu dient in erster Linie die Aussage der Vp. Mit dem Gebrauche dieser Aussagen muss man aber vorsichtig sein, da die Vp. nicht immer über ihr aktuelles Erlebnis berichtet. So erhielten wir z.B. nach dem Reizwort "Theater" einen Ausschlag nach der Unlustrichtung, während die Vp. die Aussage machte: "Sehr angenehm, ich liebe das Theater." Auf die Frage des Versuchsleiters, was sie beim Worte Theater gedacht hatte, kam die Erklärung: "Ich möchte heute so gerne ins Theater, kann es aber leider nicht." Die erste Aussage war also irreführend.

Dass man Ausschläge gegensinniger Richtung auch ohne Reizwörter erhalten kann und zu richtiger Interpretation auch ohne Aussagen gelangen kann, beweist das Tierexperiment. Da die betreffende Veröffentlichung nicht allgemein zugänglich ist, sei es hier gestattet, über diese Versuche kurz zu berichten und hiermit zugleich auf eine weitere Bestätigung der im Kongress vorgebrachten Beobachtungen hinzuweisen.[6]

Die Anordnung bestand aus einem Drehspulgalvanometer mit direkt angeschlossenen Elektroden. Diese waren Zinkplatten, die auf einer Glastafel lagen. Auf jeder Platte war ein Streifen Filtrierpapier aufgelegt, der über die Platte in der Längsrichtung etwa 10 cm. vorragte. Das Filtrierpapier war vorher in Zinksulphatlösung getaucht. Als Versuchstier diente ein einjähriger Kater. Er wurde mit isolierten Händen gehalten, während seine vorher gewaschenen und abgetrockneten Vorderpfoten auf den nassen Papierstreifen ruhten. Das Tier war anfangs misstrauisch und reagierte teils durch äussere Bewegungen, teils durch p.–g. Reflexausschläge, jedoch stets nach einer Richtung, sobald es im verdunkelten Raum das leiseste Geräusch vernahm. Erst

[5] Zu bermerken ist hierbei, dass die Richtung manchmal nicht der Erwartung entspricht. So wurde bei Affekten beobachtet, dass der Ausschlag mitunter gegen die Regel erfolgte: z.B. bei Ärger nach der Lustrichtung. Solche spezielle Fälle müssen gesammelt und untersucht werden.

[6] Ramiro Bujas, Contribution a la recherche des émotions des animaux: La loi psychogalvanique expérimentée sur un chat, *Acta Inst. Psychol. Univ. Zagreb.*, 1, 1932, (no 2), 1-20.

nach ungefähr zwei Wochen hatte es sich an die neue Umgebung und die Personen, die mit ihm arbeiteten, gewöhnt und zeigte sich nachher als gutes Versuchssubjekt. Die Ausschläge wurden an einer durchscheinenden Millimeterskala im Abstand von 1 m. vom Spiegel beobachtet. Als Reize dienten verschiedene Mittel um Empfindungen hervorzurufen und der Anruf "Mietz." Auf diesen Anruf, mit leiser Stimme gesprochen, reagierte der Kater zum ersten Mal mit einem Ausschlage nach der Richtung "—" der Skala, während alle übrigen Ausschläge bis dahin nach der Richtung "+" gingen. Von den 55 Versuchen, die in der genannten Schrift angeführt wurden, seien hier einige Beispiele zitiert. Links ist die Art des Reizes, in der Mitte die p.-g. Reaktion in mm., rechts befinden sich Anmerkungen.

Art des Reizes	P.-G. Reaktion	Anmerkungen
(1) Wasser ins Ohr	0 +7 0	Mimischer Ausdruck von Unlust.
(2) Milch	0 −4 0	Es trinkt.
(5) Sirenpfiff	0 +5 0	Zeichen von Furcht.
(6) Fleisch	0 −10 0	Es isst.
(14) Schlag auf die Nase	0 +10 0	Es zieht den Kopf zurück.
(18) Vl. hebt den Arm	0 +3 0	(Nichts vermerkt)
(19) Zucker	0 +1 0	Es dreht die Schnauze weg.
(23) Streicheln	0 −10 0	Es schnurrt.

Ausser solchem einfachen Reflexen sind auch Doppelreflexe und sonstige p.-g. Reaktionen unregelmässiger Form verzeichnet:

Art des Reizes	P.-G. Reaktion	Anmerkungen
(25) Lärm durch zu Boden gefallene Metallplatte	5 +5 0 +7 0	Zwei regelm. Reflexe nach der +Seite. Die Schwankung entsteht schnell und geht langsam zurück.
(32) Fleisch	0 +1 −2 0	Bei vielen Nahrungsreizen geht der erste Ausschlag zur Unlustseite und ist durch Überraschung hervorgerufen.

(45) Milch	0	Während des Trinkens schwankt das Galv. hin und her.
	−4	
	0	
	−6	
	−1	
	−7	
	−5	
	−1	
	0	Nachdem die Milch ausgetrunken, geht das Galv. durch den Nullpunkt zur Unlustseite, 1 mm., und kehrt später zum Ruhepunkt zurück.
	+1	
	0	
(47) Milch	0	Das Tier trinkt: Zwei gleiche Schwankungen.
	−4	
	0	
	−4	
	+2	Teller wird weggezogen.
	−4	Teller hingeschoben; es trinkt.
	+2	Teller weggezogen.
	−4	Teller hingeschoben; es trinkt.
	0	

Die Schlüsse, die der Autor dieser Arbeit aus einer grossen Anzahl seiner Versuche zieht, kann der Leser aus dieser kleinen Auswahl der Beispiele zum Teile selbst ziehen. Bemerkt muss noch werden, dass bei diesen Versuchen manchmal auch "negative" Reaktionen vorkamen (unter 5%), die mit der festgestellten Gesetzmässigkeit der verschiedenen Richtungen für Lust und Unlust nicht übereinzustimmen scheinen. "Der einzige Beweis für die Richtigkeit unserer Feststellung liegt gerade in dem Verhältnis der positiven und negativen Resultate, wo die ersteren sich als regelmässig und klar verständlich erwiesen, die letzteren aber als Ausnahmen. *Jedesmal, wenn der Versuch richtig ausgefuhrt wurde, mit einfachen und kurz dauernden Reizen, mittels einwandfreier Anordnung und hungrigen Versuchstiers, war das Resultat immer positiv.*"[7]

Vielleicht reizt es andere Forscher, solche Tierversuche in der angegebenen Weise durchzuführen und damit weitere Belege über diese Erscheinungen zu erbringen.[8]

Schliesslich sei noch darauf hingewiesen, dass vermutlich in älteren Arbeiten bisher unbeachtete Spuren dieser Erscheinungen zu finden sein werden. So ist es bekannt, dass sowohl Tarchanoff als auch Sticker Ausschläge verschiedener Richtung erhielten, und es ist sehr wahrscheinlich, dass es sich da nicht immer bloss um sogenannte diphasische Reflexe handelt. Bei anderen, die mit der Feré-Veraguthschen Anord-

[7] *Ibid.*, 18.

[8] Die zitierte Schrift von R. Bujas wird vom Psychologischen Institut der Universität Zagreb Interessenten gerne zugestellt.

nung arbeiteten, liegt die Sache schwieriger, da hier die Ausschlagsrichtung von der äusseren Stromquelle bedingt ist. Zudem sind Versuche mit Lustreizen selten zu finden. In einem Falle bei Veraguth[9] finden wir, dass ein sonst reaktionsfähiger Hund auf Darbietung von Fleisch merkwürdigerweise keine p.-g. Reaktion lieferte (das übrige Verhalten des Tieres wird nicht angegeben). Man kann wohl dieses Resultat als Indifferenz deuten, möglich ist aber auch die Erklärung, dass ein starkes Lustgefühl mit innerem Gegenstrom den Ausschlag nach der Seite des äusseren Stromes verhinderte. Anlass zu dieser Vermutung geben auch die Ergebnisse von Linde[10] welcher bei der gleichen Anordnung ein quantitativ verschiedenes Verhalten der p.-g. Reaktionen bei Lustreizen und bei Unlustreizen beobachtete. Interessant sind in dieser Hinsicht auch die Resultate, die Radecki[11] bei Benutzung des Kapillarelektrometers mit direktem Anschluss der Vp. erhielt: das Instrument gab Ausschläge nach beiden Richtungen (+ und −), ohne dass der Autor imstande gewesen wäre, eine befriedigende Erklärung dafür zu geben.

Auch aus diesen wenigen Hinweisen geht hervor, dass hier klärungsbedürftige Tatsachen vorliegen, an welchen der psychologisch Interessierte nicht unachtsam vorbeigehen kann.

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[9] O. Veraguth, *Das psychogalvanische Reflexphänomen*, 1909, 150.
[10] E. Linde, Zur Frage vom psychischen Korrelate des p.-g. Phänomens, *Bericht ü. d. VIII. Internat. Kong. f. Psychol.*, 1927, 351.
[11] W. Radecki, Recherches expérimentales sur les phénomènes psychoélectriques, *Arch. de psychol.*, 11, 1910, 231.

THE CONFUSION THAT IS GESTALT-PSYCHOLOGY

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(Founded in 1932)

Of unusual interest, surely, is the present status of what has been called *Gestalt* psychology. Introduced in a quiet but nevertheless extremely promising fashion by an Austrian group of psychologists in 1890,[1] it was thirty-four years later on proclaimed with extraordinary exaltation at Berlin.[2] Köhler writes:

> All the more important observations relating to the real units began to be made in the last thirty years, though the facts were before us thousands of years, wherever psychologists or other people looked into the world.[3]

Soon afterwards it was taken up—more modestly but even more enthusiastically—at Leipzig, and Sander tells us:

> When a long-sought and suspected connection suddenly flashes into mind in perfectly consistent formulation, when fragmentary items suddenly acquire meaning, or a tormenting chaos falls into visible order, then the emotive general condition of consciousness changes at one stroke. The confusions of feeling that accompanied the emergence of the Gestalt resolve themselves in a liberating sense of correctness and definiteness, states in which the soul and its structural affairs have attained peace.[4]

The reception of the Gestalt doctrine has, however, been curiously mixed. Most psychologists, it would seem, have been much puzzled but distinctly impressed. They appear almost fearful lest disregard of it should stamp them as being out-of-date. For the same reason, many who are decidedly opposed to it in private conversation, seem reluctant to express this feeling in print. On the other hand, a small but, apparently, increasing number of critics have uttered their sentiments very frankly indeed. First came the eminent G. E. Muller,[5] who already in the same year went so far as to accuse Köhler's writings of "misrepresentations, suppressions, and fabrications." In 1926 Bühler,[6]

[1] First came C. von Ehrenfels, Über Gestaltqualitäten, *Vjsch. f. wiss. Philos.*, 14, 1890, 249 ff.
[2] Max Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 4, 1923, 301-350.
[3] Wolfgang Köhler, An aspect of Gestalt psychology, *Ped. Sem.*, 32, 1925, 691-723.
[4] Friedrich Sander, Structure, totality of experience, and Gestalt, *Psychologies of 1930*, 1930, 188-203.
[5] G. E. Müller, *Komplextheorie and Gestalttheorie*, 1923, 1-108; Bemerkungen zu W. Kohlers Artikel "Komplextheorie und Gestalttheorie," *Zsch. f. Psychol.*, 99, 1925, 1-15.
[6] Karl Bühler, Die "Neue Psychologie" Koffkas, *Zsch. f. Psychol.*, 99, 1926, 145-159.

whose investigations of Gestalt are famous, declared that the new school was "theory-blind" and that it treated Gestalt as a "maid-of-all-work." In 1927-8 the Gestalt-doctrine was ridiculed by Rignano for its confusion of many different meanings.[7] In 1930, Oeser complains that the theory lacks just the qualitative observations that science demands.[8] Two years afterwards, Petermann—in a book now largely recognised as of standard authority—concludes that the Gestalt concepts "make all exactitude of thinking illusory."[9] Last of all, McDougall now disposes of the theory as follows:[10]

I would submit that no one of the principles of Gestalt Psychology is new, except one (isomorphism) which is, I think, demonstrably false.

Gestalt as form. In view of this surprising conflict for and against the Gestalt-psychology, the most sensible move to start with may well be an enquiry as to what this doctrine really advocates.

The common notion of it appears to have been voiced in the following words addressed by Woodworth to a popular audience.[11]

This word Gestalt carries the meaning of shape, form, mould, pattern, configuration, organization, totality—something of this sort—and the main contention of the Gestalt school is that psychology ought to be studying the pattern and organization of thoughts and actions, taken as wholes, rather than trying to analyze them into so-called elements.

But such a loose conception of the doctrine, "something of this sort," is fraught with danger, and possibly may even be one of the major sources of all the trouble about it.

The earliest formulation of the doctrine by its proponents themselves is credited to Von Ehrenfels. He took as his leading example the appreciation of melody. He pointed out that for one person to hear all the *n* tones is a different thing from *n* persons hearing one tone each. The tones he characterised as the foundation (*Grundlage*), whereas the melody itself he called the Gestalt-quality. Some of his school, notably Stumpf, have translated this Gestalt-quality as "form," and then the "foundation" becomes equivalent to "material."

Here, accordingly, we have the Gestalt doctrine in its earliest and simplest version. It essentially consists in demanding that psychology should take account, not only of mental constituents, but also of the

[7] Eugenio Rignano, La teoria della forma della nuova scuola psicologica inglese: La "Gestalt," *Scientia*, 42, 1927, 145-158, 215-228, 280-290; Die Gestalttheorie, *Psychol. Forsch.*, 11, 1928, 172-187; Zur Gestalttheorie: Antwort auf Herrn Köhlers kritische Erwiderung, *Scientia*, 43, 1928, 323-356.

[8] O. A. Oeser, Critical notice: Gestalt theory, *Brit. J. Psychol.*, 21, 1930, 73-93.

[9] Bruno Petermann, *Das Gestaltproblem in der Psychologie in Licht analytischer Besinnung*, 1931, 1-280.

[10] William McDougall, Dynamics of the Gestalt psychology, *Char. & Pers.*, 5, 1937, 131-148.

[11] Broadcasted at New York.

form in which they are put together. And to denote form so understood, the use of the word *Gestalt* appears to be legitimate enough, since "put" or "placed" is its earliest and literal meaning (*gestellt*). However, a multitude of other words can be and have been employed with much the same sense: for instance, configuration, structure, shape, pattern, scheme, system, and so forth.

Up to this point, the doctrine is such that it cannot reasonably excite opposition. It may even be regarded as opportune; seeing that it was urged in a period which had not yet fully emerged from the opposite extreme, associationism, wherein form had been greatly neglected.

However, to claim for the Gestaltism as so far specified any high degree of originality is quite unreasonable. Not only does it go right back to the beginning of psychological history, but even its personal claim of transmission is well known. Von Ehrenfels and his contemporaries got it from Brentano; the latter had it from Aquinas; and he again, from Aristotle.

Change to appearance. But never from the beginning has the doctrine of Gestalt contented itself with this certainly harmless and profitable version. Already with Von Ehrenfels we find the concept submitted to a grave qualification. He states and underlines that the Gestalt-quality is a "positive" presentational content. All later Gestaltists have followed suit. They have usually expressed themselves by saying that this content is given in *Anschauung*, a word that is classically translated as "sensorial intuition" and is rendered in Eisler's Dictionary as "the immediate apprehension of a concretely given object." The verb *anschauen* Eisler further renders as the "quiet contemplation" of the object (*ruhige Betrachtung*).

This version, no less than the former one, has linguistic support. We find that the term Gestalt came eventually to signify "how a thing looks" or "appears." But obviously, this cuts right across the preceding definition. In the earlier case, the Gestalt had included the "form" thus excluding the "material." Now, however, it embraces all sensorial "appearance," and this must include not only the form but the material also.

If this shift in the meaning of terms bred nothing more than equivocation, the objection to it would be serious enough. But there is worse than this. In limiting Gestalt to sensorial appearance, it is thereby restricted to perception; it loses contact, not only with thought, but even more so with feeling and volition. But on the other hand most, if not all, of the subsequent expositions of the theory of Gestalt have,

explicitly or implicitly, taken it to comprehend mental science as a whole. The result has been a falling between two stools. Feeling, volition, and thought are introduced in a fashion quite inappropriate to a theory of perception only, but absurdly inadequate for a theory of psychology in general.

Even more serious than this isolation of the Gestalt psychology from all that is not sensory perception is its divorce from half even of this perception itself. This trouble, too, begins already with Von Ehrenfels. For he sharply contrasts, on the one hand, the apprehension of Gestalt-quality by immediate intuition, and on the other hand, the perception of relations by means of "a peculiar activity of comparison," "a shift of the spiritual glance from one object to the other." This second procedure he dismisses as only an exercise of "intelligence."

This limitation of Gestalt psychology to immediate sensorial in-

FIG. 1. A FORM

tuition, thus excluding the comparative perception of relations would seem to have been maintained and stressed by almost all Gestaltists of all schools. And as a practical corollary, the comparative perception of relations has been amazingly left out of account.

Yet in point of fact, the two kinds of perception, intuitive and comparative, are most intimately akin. Really the former deals with relations, no less than the latter does. In fact the two supply exactly the same information. Consider, for instance, Fig. 1 above.

The spectator can either see this by "immediate intuition" as a triangular Gestalt, or else he can make the freest use of the "peculiar activity" of comparison. Otherwise expressed, he may either perceive the figure *as* a triangle, or else he may perceive *that* it is so. On completion of either procedure, he will normally arrive at *just the same objective knowledge*.

The two procedures not only lead to the same eventual result. In arriving at this, they most intimately coöperate. Consider, for instance, the following items in the test-series of Penrose and Raven.[12] One of

[12] L. S. Penrose and J. C. Raven, A new series of perceptual tests: Preliminary communication, *Brit. J. Med. Psychol.*, 16, 1936, 97-104.

the lower set of figures in Fig. 2 has to be picked out, as best fitting into the gap left in the upper set. Here, as almost everywhere, both the intuitive ("as") and the comparative ("that") modes of procedure are in operation; but they are so intimately allied and have such gradual transition from the one to the other, that they can hardly be distinguished.

Accordingly, we have here a charge of extreme gravity. The Gestalt psychology, not content with losing its grip of everything outside sensory perception, proceeds to bisect even this, retaining only a futile fraction of it.

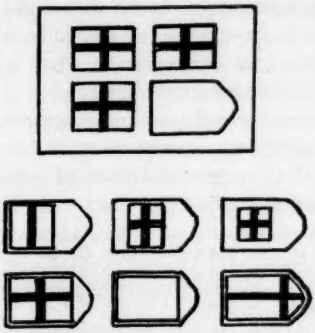

FIG. 2. A PERCEPTUAL TEST

Doctrine of one step. So far, we have traced the course of Gestaltism as followed by all the leading schools. We turn now to a point where that of Berlin abruptly and drastically departed from that of Austria. The latter school, as we saw, had conceived the perception of a gestalt as involving two distinct steps; thus in its leading example there came first the "foundation" of tones, and then the superposed gestalt-quality of tune.

Now this doubleness of process all the later Gestaltists entirely repudiated. Almost every one of them protested that the perception of the gestalt-quality was just as immediate as that of the so-called foundation itself.[13] "The forms are in no wise less immediate than their parts." And this statement about actual sensory perception was forthwith extended to its genesis. The Gestalt was taken to be immediate in the sense of having little or no dependence on previous experience.

[13] Kurt Koffka, Zur Grundlegung der Wahrnehmungspsychologie: Eine Auseinandersetzung mit V. Benussi, *Zsch. f. Psychol.*, 73, 1915, 11-90.

Thus in the age-long intense controversy between the nativistic and the empirical views of perception, the Gestalists became the most extreme partisans of the former.

Now, they assuredly cannot be blamed for the mere fact of being nativistic. This is a point on which there is undeniably room for wide divergencies of opinion. But they are not so free from reproach on the score of having adopted their nativism in a very dogmatic theoretical way, paying little or no regard to the great mass of available evidence.

Change to dynamic system. But the difficulty went further. Since the Gestaltists declined to ascribe the configuration to experience, they had to look for its origin in the brain. And if they could find no physiology at all helpful for this purpose, then one had to be invented. To explain the outward appearance of configuration, they had recourse to the hypothesis of an underlying dynamic system.

Moreover this system was soon no longer regarded as subsidiary and helpful to the Gestalt theory. It was taken to constitute this Gestalt itself. Passing over all conceptions of form, of appearance, and even of immediacy, Wertheimer writes:[14]

> There exist connections where what occurs in the whole is not derived from the nature of the single pieces and of their putting-together. On the contrary—in the pregnant case—what occurs at part of this whole is determined by the inner laws of structure of this its whole. . . I have stated to you a formula and could now end. For the Gestalt theory is just this; neither more nor less.

Wertheimer's followers have been no less insistent. Köhler writes:[15]

> This, indeed, is the most general concept of Gestalt theory: wherever a process dynamically distributes and regulates itself determined by the actual situation in a whole field, this process is said to follow principles of Gestalt theory.

So too Lewin formally defines a Gestalt as:[16]

> A system whose parts are dynamically connected in such a way that a change of one part results in a change of all other parts.

Waiving the objection that in this way the term Gestalt becomes more hopelessly equivocal than ever, and even conceding that *within moderate bounds* the use of hypothetical physiology is legitimate enough, we encounter new trouble when we proceed to consider the proposed dynamic theory more in detail. Take its climax; that which is put forward as nothing less than "the universal law of Gestalt.[17]" It runs to the effect that "any configuration will become as perfect as the prevailing conditions admit." What does this really signify? To become

[14] Max Wertheimer, *op. cit.*, *Psychol., Forsch.*, 4, 1923, 301-350.
[15] Wolfgang Köhler, *Gestalt Psychology*, 1929, 1-403.
[16] Kurt Lewin, *Principles of Topological Psychology*, 1936, 1-231.
[17] Kurt Koffka, Mental development, *Psychologies of 1925*, 1925, 129-143.

"perfect" means to arrive at some given end or ideal. It is, then, a meaningless phrase until the intended end or ideal has been specified. If so, the alleged "law" is impotent to fulfill the primary function of the laws of science; that is to say, prediction. And all this demur at the claimed tendency to become perfect may be applied also to the further asserted "urge towards completion" (*Ergänzungsdrang*). This has no definite sense until we are told *what* is to be included in "complete."

However, this very vagueness, which precludes such "laws" from telling us anything not yet known to us, does enable them by far-fetched analogies to be twisted into more or less accord with what we know already.

Change to wholeness. Many other changes of signification have befallen this chameleon term Gestalt, but cannot be considered in the present brief account. However, we must make an exception. One of these further versions cannot be passed over, seeing that it has now become the dominant feature of the whole theory.

It would seem to have been inspired originally by Goethe, who wrote as follows:

> The German has for the complex existence of a real being the word *Gestalt*. In this expression he abstracts from the movable; he assumes that something belonging together has been determined, shut off, and fixed in its character.

In these words we find nothing about form, nothing about appearance, nothing even about dynamic systems. What we do encounter—in addition to some metaphysical allusions to "real being," which need not here concern us—is the conception of the Gestalt as in itself "belonging together" but "shut off" from other things. These two features, however, are no other than those which constitute unity; for this is classically defined as that which is undivided in itself, but divided from all else. And with unity we reach the conception of a "whole," or "total;" for this is defined as the union of several things.

Accordingly, this Gestalt psychology has more and more come to be identified with 'wholeness' psychology. For instance, Köhler, explicitly claiming to follow Goethe, writes that: "In *Gestalt theory* the word 'Gestalt' means any segregated whole."[18] Again, Sander lays down that a Gestalt is a whole having as primary criterion its separateness or segregation from the rest of consciousness (*Abgesondertheit*, *Abgegrenztheit*).[19]

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[18] See footnote 15.

[19] Friedrich Sander, Experimentelle Ergebnisse der Gestaltpsychologie, *Sammelreferat*, *Deutscher Kong. f. Psychol.*, 1927.

Priority of "wholes" in time. Hand in hand with this appreciation of "wholes" there went a depreciation of "elements." This "anti-synthetic" attitude, as it has been called, was adopted very widely. Thus, Stern writes scathingly enough that "the mind thus fixed on the elements becomes blind for the individual whole." The results reached by analysis are pilloried as being only "the findings of *post-mortem* dissection."

But for the most part the anti-synthesis has been specially appropriated by Gestaltism. Koffka above all things attacks the "traditional" doctrine, according to which:

> All present or existential consciousness consists of a finite number of real separable elements, each element corresponding to a definite stimulus or to a special memory-residuum. . . . The sensation is a direct and definite function of the stimulus.

This teaching of Gestaltism is epitomised by its ablest critic, Petermann, as follows:

> The impugned point of view is one according to which perceptions are 'built-up' out of sensations. . . . And the remedy lies in an orientation, which no longer derives perceptions from sensations, but derives sensations from perceptions.

Accordingly, he says of Gestaltism that:

> Its actual theme, its nucleus conception, is the *proclamation of the anti-synthetic standpoint of totality.*

And indeed, he seems to regard this proclamation as the one scientific advance that Gestaltism has really achieved.

But surely all these doctrinal statements are sadly in need of elucidation. Which particular writings are here strangely credited with expressing "tradition"? What meaning is intended to be conveyed by those most tricky terms "real," "separable," and even "elements"? What is signified by a "direct and definite" function? Much seems to indicate that the writer really means *exclusive* function. But this interpretation seems to lead to absurdities.

More serious still is the equivocation as to what is meant by the "analysis" of a perception or other experience. Is the word intended to include such cases as attempting to analyse the perception of a musical chord by means of sounding each note separately? Such an analysis as this neither is, nor perhaps ever has been, seriously defended by anybody. Does the analysis mean "attending" to each note separately? That even this procedure is precarious has long been generally recognised. Or is the analysis at issue nothing more than one of "ideal" kind, where each constituent is only picked out by way of an abstraction? This third kind of analysis seems to have really been employed.

But just against this kind of analysis the usual attacks would seem to be pointless.

However, grave as all these equivocations are, we must for the present pass over them to a matter of still more vital moment. The Gestaltists advance, as a most fundamental theorem, that parts are derived from wholes, rather than wholes from parts. In what meaning shall this theorem be taken?

Often the meaning intended would appear to refer to the genesis of sensory perception. In this domain, the Gestaltists claim priority for the whole; they oppose the alleged traditional doctrine that the parts, in particular the sensations, are prior. But in point of fact *both* these opposed views would seem to be erroneous. Child-study shows convincingly enough that an infant begins, neither by apprehending as a unitary whole his entire sensory experience, nor by breaking this up into isolated sensations. Instead, he starts off with some middle-sized perceptual object, such as a window, a lamp, or a face; thence he works both downwards by differentiating analysis and upwards by relating synthesis. Analogous, it may be added, is all systematic study, mental or even material; the physicist starts off with smallish lumps of matter and submits these both to analysis and to synthesis.

But let us turn from the objects of a person's perception to his whole integral experiences, whether of perceiving, of thinking, of feeling, or of willing. What is prior here? Evidently, the reply must be extremely speculative. But so far, no better one seems to have been furnished than that of Spencer: a progress from the homogeneous incoherent to the hetereogeneous coherent.

Let us take one more case; it is that of exposition. Suppose we wish to learn or teach any science, do we work up from the elements to the whole, or down from the whole to the elements? Obviously, we do just as we please; the choice is not a matter of correctness, but of convenience. And if the psychologist thinks he can manage to start best from simple constituents, or from complex integrations, why not let him try? The eventual result *should* always be the same.

In short, then, this question as to the temporal priority of the whole to its constituents requires very different answers according to what it is intended to mean. As it stands it is highly ambiguous. But in no version of it does the Gestaltist doctrine appear to find support.

Priority of "wholes" in function. Let us turn from priority in time to that in function. The most important mental properties, according to Gestaltism, are those of the whole mind, and are not the mere sum of

the properties of its parts. But ordinary logic (such as those of Mill, Sigwart and many others), has long set forth the general nature of what has been named conjoint agency. Mill writes:

> It is seldom, if ever, between a consequent and a single antecedent that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents, the concurrence of all of them being requisite to produce, that is, to be certain of being followed by the consequent.

Just which antecedents happen to be appreciably effective remains in each case a problem for further solution. For instance, when a man decides to enlist in the army, his motive may include patriotism, or sanity, or despair, and so on. On the other hand, many of these or other things may *not* contribute to his decision. To say that he is moved by the situation "as a whole," if it has any meaning at all, is definitely false.

Similar considerations apply when we turn from causation to logic. This appears to happen when Wheeler gives as his leading illustration of his laws that: "All of the lines of a square derive their position and direction from the figure-as-a-whole." Now take any one of these four lines, and see on what conditions it really does depend. We find that such conditions are infinitely variable, but always limited to particular characters of the figure. Thus, suppose we possessed a square carpet and desired to know whether it would fit into a given rectangular room. One complete set of conditions would be that any two adjacent sides of the room should be equal, and that any side of the room should be equal to any side of the carpet. These two observations are for the carpet layer vital. All else is *not* involved.

Altogether this doctrine of the priority of wholes, this "proclamation of the anti-synthetic standpoint of totality," this "the actual theme" and "the nuclear conception" of Gestaltism—all this would appear to be strangely built on a platform of confusion, inadequacy, and even definite error.

Gestalt, objective and subjective. From all these and many other considerations, there seems no escape from the conclusion that there is something somewhere rotten in the state of Gestalt psychology. And this is all the more astonishing in view of the unquestionably eminent ability of its leading authors and proponents. Its numerous and grave deficiencies in broad outline are hard to reconcile with its admirable acuteness in detailed experimental execution, as also its captivating eloquence of exposition. Such authors cannot reasonably be suspected of committing one big blunder after another. More plausible would be

the suggestion of one single unfortunate slip with ill-fated widely ramifying consequences.

And something like this would appear to be the actual situation. All the troubles would seem to centre on the concept of "wholeness." What is this term really intended to mean? The Gestaltists seem either to have not asked themselves this question, or at least to have answered it with surprising lightness of heart. Thus even one of the most acute and profound among them dismisses the matter as follows: "What wholeness is we learn immediately and with unquestionable certainty in our experience."

In point of fact there exist, even within the frame supplied by the definition quoted above, wholes in great diversity. Peirce has enumer-

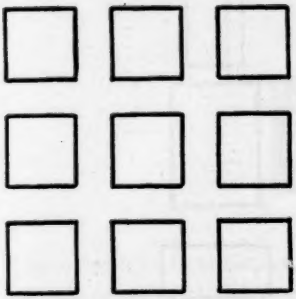

FIG. 3. REGULARLY PLACED SQUARES

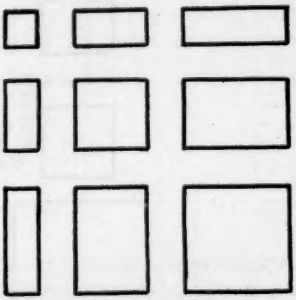

FIG. 4. REGULARLY VARYING AND REGULARLY PLACED RECTANGLES

ated no less than 43 kinds. But for our present purposes it may suffice to consider only two. These—for want of better names—may be called respectively subjective and objective.

To indicate what precisely these terms mean, let us consider Fig. 3. Quite easily we can group this, either as three rows, or else as three columns; or else, again, as two superposed crosses. In the first case, each row can be called a whole, or, indeed, a Gestalt. In the second case, each column can be so designated. In the third case, each cross. Yet in spite of the great change in appearance, the figure remains essentially unchanged. No method of grouping either adds to or takes away from the information presented. None is either true or false. None supplies us with direct aid to conduct. None could serve as a test of intelligence. In fact, save for more or less occasional vividness, this

subjective grouping is essentially the same as is implied in the word "and," and illustrated in the lines: "The time has come to talk of many things, of ships and shoes, and sealing wax, of cabbages and kings." This ever-varying, non-informative, aspect of the percept is what we here designate as "subjective."

For contrast, turn to the further perception of the very same figure as being made up of regularly placed squares. This does supply a piece of information; it does admit of being either true or false; it can easily serve to guide our conduct; it can quite normally, directly and effectively, enter into a test of what is commonly called "general intelligence." This aspect of the percept is what we have called "objective."

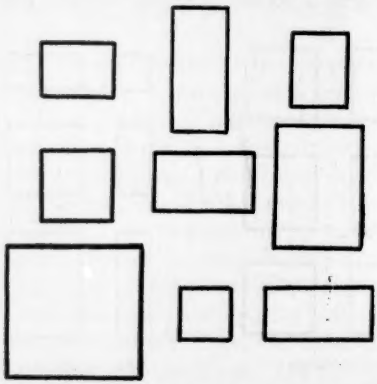

FIG. 5. DISSIMILAR RECTANGLES REGULARLY PLACED

Now turn to Fig. 4. Again we find the two aspects in the perception of a Gestalt; the one subjective, and the other objective. But the difference between the two aspects is rendered clearer by the fact that this time they are diversely accentuated. Attention is forcibly attracted to the curious objective fact of the length and breadth of the rectangles increasing respectively to the right and downwards. Absorbed in this interesting objective information, the perceiver is less free in respect of the subjective grouping. Some persons here cease to group at all. Few group the figure into superposed crosses; such a manner of perception is rendered less easy because there is a less obvious regularity involved.

In Fig. 5, all the rectangles are dissimilar; and yet the grouping can vary just as before. Moreover, since the objective characters are con-

stant, *they* cannot possibly account for the subjective perception being able to vary. At most, the objective aspect seems to act as a suggestive hint for the subjective kind. In themselves the distinction between the two aspects would seem to be fundamental and absolute.

As a third illustration, take Fig. 6, due to Thorndike. It represents a chick trying to find his way out of the maze from A into the interior Y. Here, it is quite possible either to regard each wall of the maze as a separate thing, or else to take all nine collectively as one thing. But the choice between these two does not in the least help anyone to know whether to go from A to the left or to the right.

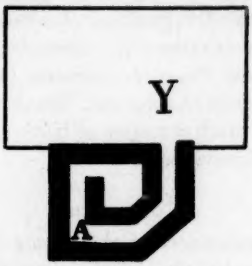

FIG. 6. A CHICK MAZE

Now, which of the two aspects of percepts, subjective or objective, has been intended by the Gestaltists? It is hard not to suspect them of having completely confused the two. With confidence, at any rate, we can assert that their main illustrations and arguments have been taken from the subjective sort, and then have tacitly been applied to the objective ones. That such a transfer should be legitimate seems inconceivable.

Any such plea as this breaks down even on the facts that we have seen already. For what possible correspondence can subsist between such utterly disparate things as the attainment of information to guide behaviour and the more or less arbitrary grouping of information obtained. The one may carry with it life and death, whereas the other seems to be only a trivial by-play.

If, however, more reason be desired for distinguishing the two kinds of wholeness or Gestalt, it may be found in the fact that only the subjective kind really involves any genuine segregation at all. Turning back to Fig. 3, we may notice that the segregating of three squares in a row is, so long as it lasts, absolute; just these three, no others

whatever, are included. And the same applies to all the groupings in any other percepts; or indeed, in any usage of the notion of "and." But turn now to the objective characters, including the very grounds which have suggested the subjective ones. They are said rightly enough to consist of such things as nearness, likeness, causation, and so forth. But these characters are never cut off sharply from one another. Instead, they ramify everywhere.

If we would have yet another fundamental difference between the objective and the subjective Gestalten, it is to be found in their respective functions. Take for example, any science; say, botany. Objectively, this constitutes a definite intimately connected body of knowledge; that of the phenomena of vegetables. But he who would teach this knowledge introduces innumerable subjective groupings and segregations. He may follow those of Linnaeus, of De Jussieus, of Humboldt, or of any more modern exponent. The choice between one system and another is not so much a matter of truth and falsehood, as rather of convenience and effectiveness.

Conclusion

After all this consideration of the nature and doings of Gestalt-psychology, let us conclude by drawing up a summary balance sheet.

In general it aimed at a vigorous reaction against the doctrine of associationism, and with this view strenuously urged the claims of gestalt-quality against those of sensation. But against this asset of good intentions must be set the deplorably equivocal use of the term "Gestalt," whereby the whole doctrine has been befogged.

The extraordinary result has been a psychology divorced from most of what belongs to this science. In particular, it has lost effective contact with all volition, all feeling, and even all thought.

Even within the domain of sensory perception itself, it has fettered itself within the limits of "passive contemplation" of form, to the exclusion of all active cognition of relations; a divorce like that of as trying to walk on one leg.

Deeper than all these defects, however, and mainly responsible for them, has been one great paralysing confusion; that between the objective matter regarded and the subjective manner of regarding it. Under such names as "Gestalten," "wholes," "totals," "configuration," "segregation," "shut-offness," all genuine processes of knowing have been replaced by mere trivialities of grouping. To fill the ensuing void in psychology, many dynamic theorems have been devised;

but these, besides being almost destitute of factual basis, are so vague that—like the ancient oracles—anything can be read into them; nothing read out of them.

As definite attainment of the entire movement,[20] then, there remains little else than a harvest of experimental results within a fragmentary range of sensory perception. The experiments themselves may be credited with highly ingenious construction, but very forced interpretation.

[20] A bibliography of the works on the subject down to 1926 has been given by the present writer in The new psychology of 'shape,' *Brit. J. Psychol.*, 15, 1925, 223-225. Of the more important later works not listed in that article may be specially mentioned: Harry Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223; R. H. Wheeler, *The Laws of Human Nature*, 1931, 1-232; Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 1-720; and the journals, *Psychologische Forschung* and *Neue Psychologische Studien*.

THE SIGNIFICANCE OF BEHAVIOR ACCOMPANYING CONDITIONED SALIVARY SECRETION FOR THEORIES OF THE CONDITIONED RESPONSE

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(Founded in 1932)

The phenomena of conditioned response action have been of interest largely because they constitute the central empirical basis and factual point of departure for current associationist systems of psychology of the behavioristic variety. The more general principles of these systems antedate any set of experimental facts. In their present precise and differentiated form, however, they arose from attempts to explain the facts discovered by experimental application of the conditioned response technique, especially to the response of salivation.[1] The ultimate value of these systems lies undoubtedly in their capacity to explain deductively the whole range of psychological phenomena, and thus they can not be finally judged at present. We are not here concerned with the validity of the deductions already so ingeniously elaborated, but rather with the adequacy of these systems' fundamental picture of the learned modification of mind or brain to the group of phenomena constituting their most direct empirical foundation—conditioned response behavior itself.

No other systems of psychology have selected these particular phenomena as the basic set to which their explanatory concepts are most closely geared. But some theorists, such as McDougall and Tolman, have attempted to explain conditioned response phenomena in terms of other concepts. The nature of these concepts was determined by the characteristics of the apparently more complex forms of purposive and insightful behavior for whose explanation they were initially formulated. As applied to the conditioned response they yield a more complex picture of its structural basis and imply the existence of additional features in the behavior itself. Some evidence of these

* This investigation was aided by grants from the Research Council of Duke University.

[1] For explicit recognition of this historical fact, see C. L. Hull, Learning: II. The factor of the conditioned reflex, *Hdbk. Gen. Exper. Psychol.*, 1934, 410.

features may be gathered from the present experimental literature.[2] For the most part, however, and especially with regard to the classic conditioned salivary response of Pavlov, the occurrence of these features is largely assumed by these alternative theories for whose support everyday naturalistic observations are relied upon.[3]

Associationist conceptions. All of the more elaborated conditioned response theories are of the substitute-stimulus type.[4] These vary among themselves in several respects, most importantly, perhaps, in the way they treat the problem of behavioral *complexity*. According to the strict Pavlovian view behavior consists of large number of component reflexes. These can be considered as independent response elements which are independently conditioned to the stimuli occurring. The complexity of the conditioned response behavior consists in the multiplicity of constituent responses, and the complexity of its structural basis lies in the manifold independent connections between the conditioned stimulus and these response elements.[5] Investigation of the properties and conditions governing the acquisition of the response elements should reveal the laws governing the whole. Thus by a process of response summation and inhibition the total complex may be deduced. Pavlov chose for study the salivary rather than the muscular components because of the ease of quantitative measurement and smaller temptation to "interpret them in anthropomorphic terms."[6]

The frequent use of the term conditioned response rather than *reflex* implies a rejection of this response-element view, and it is explicitly so rejected by Hull, Holt, Guthrie and others. Holt[7] sets forth a picture of organized response patterns (neural integrations at a higher level) which may be conditioned as such. Guthrie simply assumes that "whatever complex of movements was in process" is conditioned.[8] Hull also rejects the "building block" theory,[9] yet his highly atomistic diagrams full of fractional responses independently connected strongly suggest that in practice his own theory is not essentially different.

All these theories, however, are alike in the following two points. They all assume

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[2] See E. C. Tolman, *Purposive Behavior in Animals and Men*, 1932, 319 ff.; and E. R. Hilgard, The nature of the conditioned response: I. The case for and against stimulus-substitution, *Psychol. Rev.*, 43, 1936, 366–385; II. Alternatives to stimulus-substitution, *ibid.*, 547–564.

[3] See William McDougall, *Energies of Men*, 1933, 212 ff.

[4] As representative see: E. R. Guthrie, *Psychology of Learning*, 1935, 26 ff.; E. B. Holt, *Animal Drive and the Learning Process*, 1931, 24 ff.; C. L. Hull, A functional interpretation of the conditioned reflex, *Psychol. Rev.*, 1929, 36, 498–511.

[5] This hypothesis may be tested in two ways: (1) One of the asserted response elements, say the secretory, may be elicited separately and conditioned in isolation from the other components of behavior. This has probably never been done or even attempted except by neglecting to observe or record the other components. Thus even the possibility of this view has never been directly demonstrated. (2) The conditioned response behavior may be elicited under systematically varied conditions. If the different components (say the secretory and the overt) consistently vary together in a biologically significant manner throughout these changes in conditions, the hypothesis of independent connections becomes increasingly improbable. Tests of this sort are described below.

[6] I. P. Pavlov, *Conditioned Reflexes*, (Eng. trans.), 1927, 17 f.

[7] Holt, *op. cit.*

[8] Guthrie, *op. cit.*, 42.

[9] C. L. Hull, The mechanism of the assembly of behavioral segments in novel combinations suitable for problem solution, *Psychol. Rev.*, 42, 1935, 227 f.

that the basic permanent modification is the establishment of an association, connection, or tendency to excitation between the conditioned *stimulus* system and a *response*, regardless of how the complexity of the latter is conceived, or how serially complicated the connection itself may be. They also assume further that the response conditioned is substantially identical with that which occurred during learning. This is considered by these authors to rest upon empirical fact, but it is also demanded by their theories. In fact the latter would not even be testable unless the response remained similar within identifiable limits.

These limits of identity have, however, never been satisfactorily defined by any of these theorists. The conditioned response is merely asserted to be substantially, essentially, or in gross features, like the unconditioned. This has tended to make the theories invulnerable to attack. A recent impartial evaluation of the evidence leads to the conclusion that in any usable meaning of the term "identical" the conditioned and unconditioned responses so often fail in identity that the substitute-stimulus theory is clearly inadequate as a general theory of conditioning.[10]

Yet a large body of experimental evidence remains in which the unconditioned and conditioned responses are reported as sufficiently similar to afford support to these theories, which, of course, can always be supplemented by additional assumptions to account for the presumably atypical instances in which non-identity appears. Thus the vast array of data which has been accumulated in the Russian laboratories concerning the conditioned salivary response would seem to be left as a valid empirical foundation for this theory.[11]

A number of years ago it seemed to the writer that, with differential implications of the substitute stimulus and other theories in mind, a reëxamination of this particular response would be highly desirable. The salivary response in the dog was chosen for experimental analysis because historically it was the first to be systematically studied; the basic Pavlovian principles accepted by most conditioned response theorists were initially derived from its investigation; it still remains the response about whose conditioning most is known; and it is an excellent representative of the type of 'simple' response whose apparent 'reflex' character seemed to justify the use of the term conditioned reflex, and the belief that it provided a physiological explanation of learning.

Before presenting the observations that have resulted from this plan it is desirable to set forth clearly the alternative conception to be tested. The precise formulation stated below has grown out of prolonged puzzling over the behavior observed but the writer freely confesses an initial partiality, based on general psychological grounds, for the type of concept employed.

The sign-urge conception. Various forms of this type of explanation of conditioned responses have been put forward.[12] The form held by the writer will be outlined below.[13] We are concerned with the conception of the permanent structural change produced, not with the process of acquisition.

According to the sign-urge conception, the essential structural modification consists

[10] Hilgard, *op. cit.*, 547.

[11] "As frequently pointed out in the preceding section of this chapter, the conditioned reaction as a rule resembles very closely the unconditioned reaction. Indeed, in some situations, such as those of salivary secretion, it is at present nearly or quite impossible to distinguish the two except for their relative strength and latency" (Hull, *op. cit.*, *Hdbk. Gen. Exper. Psychol.*, 1934, 431).

[12] Tolman, *op. cit.*, 330 ff.; McDougall, *op. cit.*, 212 ff.

[13] Briefly outlined previously; see Karl Zener, Another attempt at an experimental analysis of the psychological nature of conditioned responses, *Psychol. Bull.*, 30, 1933, 725.

in a reorganization into some kind of functional whole of the perceptual systems (relatively afferent, if you will) corresponding to the conditioned and unconditioned stimuli;[14] and in the functional relation of this organized system to the urge or tension system originally excited by the unconditioned stimulus. It assumes that arousal of this reorganized perceptual system, by presentation of the conditioned stimulus alone, results in expectation of the unconditioned stimulus. The functional relation between this active system and a tense urge system releases and motivates behavior directed (as approach or avoidance) toward the signified goal. After acquisition of the conditioned response, the *intensity* and *persistence* of the behavior will vary primarily with the state of the urge system involved and with such situational factors as the specific kind and magnitude of the goal object, its distance from the organism, etc. The *direction* of behavior is determined by the organization of the environment-organism field (as perceived, inferred, expected).[15]

According to the sign-urge conception the *complexity* of conditioned response behavior is the complexity characteristic of all dominant reactions of an organism. This complexity is not thought of as analyzable into either constituent response elements or into a series of complex but definite individual reactions. Behavior is considered to be directly determined by the organization of the whole psychological organism-environment field. For the determination of the conditioned response the functionally most significant parts of the field are the sign-significate (goal) organization; means and barrier objects and behavioral supports, in their spatial, temporal, causal interrelations; and the functional relations between these objects (or processes) and the urge or tension systems of the organism. These *direct* determinants are, in turn, determined by the complex processes of perception and the complex of systemic and other processes regulating urges. These latter processes thus become indirect determinants of the conditioned response (they constitute essentially another set of problems). According to this view then, the essential structural change in conditioning is that underlying the sign-significate organization and its dynamic relation to the urge system involved.

According to this conception the conditioned response behavior will vary with any variation in the organization of the psychological field. Whether it is identical or similar to the unconditioned behavior depends entirely upon this organization and cannot be predicted a priori. The conception postulates that within the limits set by the perceptual organization of the field and the plasticity of the organism the behavior will always be *appropriate*. The concept of appropriateness cannot be considered apart from the general conception of the goal directedness of behavior. If it be assumed that all behavior is directed toward (or away from) those objects (goals) in the psychological field which stand in a definite dynamic relation to corresponding urge systems within the organism, then appropriate behavior may be defined as that which brings the organism closer to (further from) the goal or goals corresponding to the urge systems then tense or active.[16] The preciseness with which the appropriateness of behavior can be determined thus be-

[14] For convenience the conventional terminology is employed instead of the more accurate designations conditioned stimulus-object, etc., (see below, p. 400).

[15] Such terms as distance, direction, intensity, tenseness, etc., are here meant in a psychologically defined and functionally determinable sense. Fortunately in the situations discussed below there is a relatively close correspondence between the physical and psychological determination of these variables. A rough identity may thus be assumed in the descriptions.

[16] Or if this is not physically or physiologically possible: preparatory or expectant behavior oriented in the direction of the goal; or restless behavior which, although not directed toward the goal, does not increase the distance from it.

comes identical with the preciseness with which behavioral approach and avoidance can be. This, in turn, depends upon the precision with which the concepts of direction, distance, force, and the like, can be conceptually defined, coördinated with satisfactory functional or operational procedures, and thus applied to the analysis and prediction of concrete situations.[17]

It is true, as Hilgard has recently insisted, [18] that the term appropriateness has often been loosely used. Although he grants the general applicability of the general kind of theory here outlined he claims that it is not vulnerable to disproof, and therefore lacks scientific status. Hilgard's criticism may best be evaluated by determining the precision with which a definite and limited range of behavior (within the total physiologically possible range) can be determined as appropriate in the sense defined. If such a range can be determined, then regardless of whether the specific kinds of alternative possibilities within it can as yet be predicted accurately, the hypothesis has predictive value and can be tested. This problem will be discussed after presentation of the behavioral evidence.

Methodological Considerations

The relative inadequacy of available experimental facts to differentiate decisively between the alternative theories of conditioning is due largely to the circumstance that the traditional method reflects the substitute stimulus conception held by the original Russian investigators, and is thus not well designed to reveal facts conflicting with it. Thus with the salivary response, although concomitant occurrence of overt behavior has been reported,[19] the fact that it has not been described in any detail reflects the opinion that it is incidental and irrelevant to the response selected for study. The traditional selection of simple reflex-like responses for study, and of graphic methods of recording has necessitated a higher degree of constraint (achieved either by physical means or by training) than any other learning method. Thus the behavior which is allowed to occur, other than the selected response, is extremely limited and is frequently so slight in magnitude that its psychological interpretation is highly ambiguous. This is particularly true of the determination of the *direction* of the behavior, since, psychologically, direction can only be determined with assurance by letting the train of behavior complete itself (*i.e.* reach its goal).

Methodologically the basic aims in the present series of experiments have been to obtain as complete a description as possible of the overt

[17] See Kurt Lewin, The conceptual representation and the measurement of psychological forces, *Contributions to Psychological Theory*. (Forthcoming.) This monograph constitutes the most sustained attempt to elaborate a set of concepts fulfilling these criteria. (Seen in manuscript by the writer.)

[18] Hilgard, *op. cit.*, 554 f.

[19] Pavlov, *op. cit.*, 171.; also The reply of a physiologist to psychologists, *Psychol. Rev.*, 39, 1932, 91–127.

behavior occurring during manifestation of conditioned salivary responses built up by the usual Pavlovian technique; to elicit the behavior both under the conditions of training and under *systematically varied* conditions. The latter requirement is demanded by acceptance of the vew that no amount of behavior occurring under conditions identical with those of training will yield complete information concerning the nature of the structural change that occurred during learning, or is likely to yield crucial evidence bearing differentially among alternative conceptions of this change. The former requirement was approximated by simultaneous observation of the secretory and overt behavior of the animal. No graphic records of overt behavior were obtained but it is felt that the demand for 'objectivity' was met by filming numerous experiments. The availability of these photographic records for inspection by loan or purchase of duplicates offers the opportunity of independent check on the accuracy of most of the behavior described below.

The increasing practice of filming animal behavior under experimental conditions (especially behavior occurring during initial exposure to new conditions) is of more than illustrative value. The methodological advantages of supplementing direct observation or graphic records by permanent photographic records are great. But the possibilities of making such films accessible for examination by fellow research workers (as well as demonstratively for instructional purposes) have not been sufficiently exploited. It is the writer's opinion that such a practice followed more routinely would be a valuable contribution to the technique of scientific reporting. Behavior so recorded can occur with fewer restraints, its *directional features* are more adequately recorded, and the data thus made available may be more significant than any analytic and selective verbal or graphic record could be; they also provide the opportunity for an a posteriori check by others of certain hypotheses that may not have occurred to the original experimenter.

Method and Procedure

The experimental set-up and procedure are described in detail elsewhere.[20] The training table and all stimuli are located in an inner, relatively soundproof chamber placed within a larger observation room which contains the recording and stimulus regulating instruments. A one-way vision mirror permits continuous observation of the animal's behavior from the outer room. The dog reaches the training table by walking up stairs placed between it and another table parallel to it against the side of the chamber opposite the observation window. The second table was used only for the behavioral tests to be described at the end of the paper. The dog is loosely secured on the experimental table by a strap beneath its belly and a chain attached to its collar.

For clarity of discussion the generalized descriptions of overt behavior will be limited to reactions in which the conditioned stimulus is a bell (4 in. in diam., producing a clang 65 db. in intensity) placed on the wall of the chamber, 5 ft. in front and 2.5 ft. to the right

[20] Karl Zener, The conditioned salivary response: An analysis of the hunger factor. (In preparation.)

of the dog's head, slightly above its level. The dogs localized the source of the sound rather precisely. The food used for reënforcement consisted of 4-6 small pieces of dog biscuit slid into a food-pan just beneath the dog's nose. Acid used for reënforcement (4 cc. of 0.25% hydrochloric) was introduced into the dog's mouth through a small metal tube connected by a tube-system to a source of supply in the outer room, from which the flow was regulated. Secretion was measured by means of displacement of water in a tubing system running from a disk placed over the external opening of the parotid duct on the dog's cheek to a capillary tube in the outer chamber placed underneath the observation window.

The traditional Pavlovian training technique was used. The conditioned stimulus at first preceded the unconditioned only by a second; the interval being gradually lengthened to a standard 15-sec. delay. The conditioned stimulus then occurred for 25 sec., with food presented after 15 sec. The behavior to be described below is that occurring during this 15-sec. period, after the completion of training. The experiments were carried out in the morning, from 20–25 hr. after the previous daily ration.

Results: I. Behavior under the Conditions of Training

(1) *Behavior occurring with a standard state of hunger.* The differences in the behavior of individual dogs preclude any simple generalized description of standard behavior accompanying the conditioned salivary secretion. The responses, however, all fall within a definite range of behavioral possibilities which is far less than the total number physically and physiologically possible. Every type of behavior occurring may be said to be equally characteristic of the situation, and although individual dogs show predilections for one or another of these types, most of them occasionally behave in almost all of the ways indicated. The types of behavior are described below.[21]

(*a*) *Before conditioning.* The unconditioned responses to the bell and to food are much more similar among different dogs than the corresponding conditioned responses. The animal turns quickly and fixates the bell. At the fall of food the dog lowers his head into the food-pan, ingests one or more of the biscuits, and then, either keeping the nose in the food-pan or raising it somewhat directly over the pan, chews or gulps down the food; this behavior is repeated until the food is all eaten.

(*b*) *After conditioning.* After conditioning, the dog may look at either the bell or the food-pan during the entire delay period. A few of the older, long-trained and stable dogs may look at the bell immediately and continuously until the food drops, or until a second or two before. This is not likely to occur with high hunger in any of the dogs. Converse behavior typical of some dogs and frequent in all dogs in a state of high hunger is an initial glance at the bell (even this is sometimes omitted) and then a constant fixation,

[21] Because of limitations of space and in the interest of clearness of presentation, only the behavior accompanying the conditioned secretion to a bell reënforced with food will be described in detail. The most important psychological features of the bell as a stimulus were its auditory nature and moderately loud intensity (both tending to make it a rather compelling stimulus), its ease of fixation, its location in such a position that it could be easily oriented toward, and (methodologically) the ease of distinguishing between orientation toward it and toward the food-pan.

during the rest of the interval of the food-pan or the opening of the chute which the food will come through (Figs. 2 and 5). In some cases the nose is kept almost touching the bottom of the pan. More frequent, however, are transitional cases in which the bell is first fixated, for a varying number of seconds, and then the food-pan for the remainder of the delay period. The shift in orientation may occur at any time during the 15-sec. interval. More frequent still is *oscillation* between bell and food-pan. Occasionally there may be 6 or more successive glances from bell to food-pan and back during the interval. These may be smooth and 'deliberate,' or jerky and 'hysterical' in appearance. The rate of oscillation frequently increases toward the end of the period; or it may give way during the final seconds to a constant fixation of the food-pan. The directness of fixation varies considerably as does the absolute magnitude of the oscillating movements. Thus there may be only a slight inclination of the head accompanying fixating movements of the eyes, or the head may be pointed directly at the bell and then down directly into the food-pan.

(*c*) *Compromise orientations*. Compromise orientations constitute a frequently occurring group of responses. The dog may hold his head at an angle between the bell and food-pan. Frequently this orientation is accompanied by a rotation of the head and neck, a 'cocking' movement, reminding one of the side-wise motion of a chicken in fixating an object on the ground. This enables the dog to orient himself toward both of the significant objects simultaneously. A given side-wise cocking may be maintained throughout the interval or it may be reversed or alternate with a more direct fixation of either bell or food-pan. The ears may move. Those dogs capable of so doing may turn their ears to localize the source of sound, and, with intermittent sound stimuli, twitch them at each presentation.

A rather persistent feature of all this behavior is a tendency to shift the focus of regard from the conditioned stimulus to the food-pan toward the later part of the stimulation interval. This may occur by a simple and permanent shift from the one to the other, as mentioned above; or by a relative increase in amount of time spent looking at the food-pan during oscillation; or by a gradual sinking of the head into the pan, even though not looking at it directly.

(*d*) *Bodily orientations*. Not all of the behavior is restricted to head movements. Changes in bodily orientation and position also occur, though less frequently. In general it may be said that if the dog is initially in a position which is not a convenient one in which to stand while eating out of the food-pan, there is a bodily readjustment to such a position before the food falls. A striking phenomenon in a few dogs is a small but definite movement of approach toward the conditioned stimulus after presentation, followed by a backing up later to a position to eat.[22]

Chewing and licking responses occur. Contrary to what might be expected from the literature and from theory, [23] however, they never occur as a sustained response; in most dogs they appear only irregularly; in some very infrequently. Thus the anticipatory reactions are directed either toward the conditioned stimulus or food-pan, but only infrequently toward food in the mouth.

Various reactions occur which are not clearly directed toward objects in the environment. Thus stepping or 'stamping' movements of the fore and hind feet occur at times in nearly all dogs, very frequently with some. Sometimes these result in a slight approach toward the conditioned stimulus, or in a new position which will be more convenient

[22] This bears upon Tolman's suggestion that the conditioned stimulus may be a subordinate goal.

[23] Pavlov, *Psychol. Rev.*, 39, 1932, 95.

for eating, and these should more properly be grouped as components of a directed action. But frequently they appear to be mere *restless* motions resulting in no change of position, often occurring in a small outburst within the first 5 and again within the last 5 sec., especially when the dog is quite hungry. Tail wagging occurs in a few dogs, but generally only at the beginning of the conditioned stimulus, or when the experimenter is heard outside. Panting occurs occasionally, but much more frequently in the sated state.

Only occasionally does behavior occur which does not fall within the range just outlined. Sometimes after glancing at the bell a dog may look straight ahead or even somewhat to the left (bell is to the right) apparently fixating nothing. This response occurs only in older, experimentally sophisticated, dogs and never in a state of high hunger. It appears to lack psychological direction rather than to be directed *at* some object in the environment. Some dogs rest their heads between stimulations on a small shelf to the left of the food-pan. One such dog only glances toward the bell and then very slowly slides his head over the food-pan without looking into it. In general it may be said that barring extraneous external stimuli responses clearly directed at objects in the environment other than bell or food-pan, or directed negatively away from these, occur very rarely in the hungry state.[34]

(*e*) *Acid behavior.* Only a brief indication of the acid behavior can be given. Psychologically it differs from the food behavior in being a withdrawal instead of an approach reaction, and in that the goal object (acid) appears within the animal's mouth. This circumstance makes the directional features of the resultant behavior much less clear than they would be if (in closer analogy with food) the acid could be seen approaching from an outside position before being placed in the mouth. This feature may be related to the fact that the conditioned and unconditioned behavior is much more similar than in the case of food.

The *overt directed* behavior accompanying the conditioned acid secretion differs strikingly from that based on food. The food-pan is practically never looked into. After fixation of the conditioned stimulus the dog may turn his head toward the side on which the acid will be given up to 90° or more (Fig. 6) and lower his head into the position which is taken after the acid comes (Fig. 1). Some dogs swing from one side to the other and back again before the acid comes. In one dog there is a definite, sometimes even violent pulling back against the chain away from the conditioned stimulus (a light, directly in front) accompanied by general struggling movements. The most universal response is licking the mouth. In some this starts shortly after the onset of the conditioned stimulus and persists until the acid. In others there may be only several licks during the interval. Frequently the licking speeds up and becomes more vigorous toward the end of the fifteen seconds. Restless behavior is less marked in most of the dogs than with the food reënforcement, except in the dog mentioned above whose struggling behavior was pronounced.

(*f*) *Restraints removed.* The *differences* in the directional character of conditioned food and acid behavior is more pronounced when the experimental restraints are removed. For several days four dogs were tested unstrapped on the training table. In no trial did

[34] The precise character of the reaction and the proportion of time spent in fixation of the conditioned stimulus varied, of course, with the latter's nature and with its localization. A buzzer on the wall to the left produces behavior similar to the bell. A pipe (in front), an intermittent telegraph clicker, and a moving card (to the right) tend to be fixated less precisely and much less persistently. A bell underneath the table was not oriented toward precisely nor persistently.

any of the four dogs move *away* from the food-pan upon presentation of the food reënforced conditioned stimulus (the behavior was undistinguishable from that with restraint except for an exaggeration of the initial slight approach to the bell in one animal). With presentation of the acid reënforced conditioned stimuli, however, avoidance behavior became more pronounced. In *none* of the trials was there any increase in orientation toward the food-pan; in some trials no gross change occurred; but some trials occurred, with three of the four dogs, in which, before the 15-sec. delay period was up, the animal walked definitely *away* from the food-pan, either to the edge of the table or down the steps, or jumped off the table.[25] It is to be stressed that these trains of behavior had never occurred to the unconditioned stimulus during reënforcement (given under restraint) *before* they appeared to the conditioned stimulus.

What is the differential bearing of the behavior just described upon the theories under consideration? Except for the component of salivary secretion the conditioned and unconditioned behavior is not identical. (a) During most of the time in which the bell is reënforced by the presence of food, chewing generally occurs with the head raised out of the food-pan but not directed either at the bell or into the food-pan, or at any definite environmental object. Yet this posture practically never, even chewing only occasionally, occurs to the conditioned stimulus alone. Despite Pavlov's assertions, the dog does not appear to be eating an imaginary food. (b) Nor is the behavior that does appear an arrested or partial unconditioned reaction consisting of those response elements not conflicting with other actions. It is a different reaction, anthropomorphically describable as a looking for, expecting, the fall of food with a readiness to perform the eating behavior which will occur when the food falls. The effector pattern is not identical with the unconditioned.[26] (c) Movements frequently occur which do not appear as part of the unconditioned response to food: all the restless behavior of stamping, yawning, panting.

It is clear that the behavior that occurs to the conditioned stimulus is not "the complex of movements" that occurred to the unconditioned stimulus. The deviations from this behavior, though not *gross*, are highly significant. They are variations consistent with the sign-urge conception—behavior appropriate not to the *presence* of food in the food-pan, but to the expectation that it will appear. Both restless and

[25] This response occurred in one dog 3 sec. after the first presentation of the acid conditioned stimulus on a day after one devoted exclusively to acid reënforcements. This trial was preceded and followed by presentation of a conditioned food stimulus to which only approach behavior appeared.

[26] It is significant that when the posture is most similar to an arrested unconditioned reaction (the head held down in the food-pan with nose almost touching it) the further movements of chewing practically never occur (the attitude is one of tense, but controlled expectancy).

directed movements are consistent with this hypothesis. It is to be stressed that these represent only a small fraction of the total possibilities of behavior, most of which would be inconsistent with the hypothesis (and many of which do occur in the same physical situation with different conditioned stimuli, with acid reënforcement, and with the internal changes in state reported below).

II. Behavior under Changed Conditions

To be contrasted with the behavior which occurs under the conditions obtaining during training and which we have just described, is the behavior that appears when conditions are changed. We shall consider three types of change: two of internal state (hunger and extinction), and one of external relationship (position of the animal).

(*1*) *Behavior with a change in hunger.* We now consider the behavior of the animals when hunger is decreased, *i.e.* when the animals are satiated; and then we shall describe the differential effects of satiation.

(*a*) *Decrease in hunger.* Decreased hunger produced by previous feeding results in a decrease in the conditioned secretion. The extent of this varies with the dog and with the degree of satiation. If the food used for reënforcement is presented in a pan, a cup at a time, and the dog allowed to eat to refusal, a moderate degree is attained (state of pan-satiation). The dog will then generally eat the food when it appears as reënforcement following the conditioned stimulus. The change in behavior may be slight, consisting chiefly in a decrease in alertness, intentness, precision of fixation, and general muscular tenseness. Though slight, this difference is generally definite and detectable to one familiar with the animals. Anthropomorphically it suggests a lessened interest in the situation.

Usually, however, the change in behavior is more gross. There is frequently delay in looking into the food-pan, decrease or absence of oscillation, often even in looking at the conditioned stimulus. Decrease in precision of fixation may be more pronounced, the head movements being reduced in amplitude. Thus in the hungry state the nose may be kept literally in the food-pan, whereas in the sated state the head may be kept over the pan but held high without looking into it; or the head may be kept at one edge of the pan instead of over the middle. There are few restless movements. Few new movements appear.

If, when the dog no longer eats biscuits from the pan, they are tossed one at a time into the food-pan, quite a few more are frequently eaten. When the dog ceases to eat biscuits presented in this manner he generally refuses them also as reënforcement following the conditioned stimulus (stage of hand-fed satiation). The decrease in conditioned secretion is now invariably marked, falling (depending on the dog) to an average of 5–25% of the secretion when hungry, and frequently there appears no secretion at all.[27]

At this stage the behavioral contrast with the hungry state is also more marked. The animal may not look toward either conditioned stimulus or food-pan until food falls and occasionally not even then. Or he may only glance toward the conditioned stimulus by a movement of the eyes without a change of head position. With these decreases in ampli-

[27] Zener, The conditioned salivary response: An analysis of the hunger factor. (In preparation.)

tude of movements occurring in the hungry state, new movements may appear. The animal may look off to one side or the other (physically *away* from the food-pan). Thus frequently, with one motion of the head, the dog will rapidly nose the food and perhaps look away at an angle of 90° or more, remaining so until after cessation of the conditioned stimulus or until the food is removed a little later. An increase in restless movements is frequent.

In striking contrast to the just described correlated change in conditioned secretion and overt behavior to stimuli reënforced with food is the *lack* of consistent change in either secretion or behavior with stimuli reënforced with acid.[28]

(*b*) *Differential effects of satiation.* If a dog is fed to refusal on one food he may yet remain eager to eat other foods. If several different conditioned stimuli are reënforced with different foods and the animal be sated on one of them, then the conditioned secretion to the stimulus reënforced on this food will markedly decrease, while that to other stimuli will drop or remain high, depending on whether the dog refuses or remains eager for the particular food they signalize. This correlation between the secretional and behavioral changes is particularly significant. It is best shown by presenting the protocol of a single experimental session which is typical of a whole series with this dog. It requires no detailed comment.

DIFFERENTIAL SATIATION[29]

(Ed., Dec. 11, 1933)

Trial 1. Bell. Looks into food-pan immediately and continuously. (C.S.=14.6 cm.)

Trial 2. Bell. Same behavior. (C.S.=24.2 cm.)

Trial 3. Pipe. Looks up toward pipe, then directly into food-pan. (C.S.=23.4 cm.)

Trial 4. Pipe. Same behavior. (C.S.=26.2 cm.)

Fed three cups of Spratt's ovals in a pan, a cup at a time. Refuses fourth cup.

Trial 5. Pipe. Head is on shelf. At pipe looks immediately into food-pan. Noses ovals from 10th–17th sec., then eats. (C.S.=4.2 cm.)

Trial 6. Pipe. Same initial position; same behavior (C.S.= 22.0 cm.)

Refuses a cup of ovals offered in a pan. When tossed into the food-pan one at a time he takes another cup before refusing. Then takes 4 half-ovals offered in the hand, refusing when they are tossed into the food-pan.

Trial 7. Pipe. Head is on right lamp. At pipe stamps with hind feet twice but does not move head either during pipe alone or when food falls. At 15th sec. noses food in food-pan briefly but does not eat. Rests head on shelf at 18th sec. (C.S.=1.5 cm.)

Trial 8. Pipe. Head is on left lamp. No head movements at pipe; remains motionless except for curving of tail. No movement of head at fall of food; stamps two hind and one front foot, then is quiet. At 22nd sec. moves head half-way toward food-pan but pushes head over screen at right at 25th sec. without eating or nosing food. Then attempts to climb over the screen (vigorous escape behavior). (C.S.=1.0 cm.)

Trial 9. Bell. Is looking to left of food-pan. At bell looks immediately into food-pan and continues until fall of food. Eats after slight hesitation. (C.S.=7.0 cm.)

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[28] Zener, Differences between internal stimulus and urge as motivational concepts in learning: An experimental test with conditioned salivary responses based on food and acid. (In preparation.)

[29] Bell, reënforced with Sergeants biscuits, located under the table; pipe, reënforced with Spratt's ovals, placed in front of dog. Conditioned stimuli presented for 20 sec., with food at 10th. sec. Two lamp boxes, one on each side of the food-pan. A wire screen runs parallel with the dog about a foot to his right. The conditioned secretion (C.S.) measured during the 10 sec. presentation of conditioned stimulus alone is reported in centimeters displacement (1 cm.=1.25 drops of 0.01 cc. each). When the reënforcing food is not eaten it is removed at the 30th sec.

Trial 10. Bell. Head is on left lamp. Same behavior. Eats without hesitation. (C.S. = 16.5 cm.) (Note that hang-over effect from pipe in previous trial expresses itself both in lowered secretion and in hesitation in eating food when it falls.).

Trial 11. Pipe. Head is on left lamp. At pipe jerks head off lamp but only lowers nose toward food-pan about an inch. Continues in same position until food falls, then looks rapidly into food-pan and out without eating. Rests head on right lamp. Then licks top of lamp and own chops for several seconds. Tail is curved and relaxed—restless behavior; note effect of hang-over from Trial 10, comparing this with Trials 7 and 8. (C.S. = 1.6 cm.)

Dog is removed from stand and taken into outside corridor, where there are placed two pans containing half cups of 'ovals,' and 'sergeants.' Ed smells the 'ovals,' then eats the 'sergeants,' finishing in 35 sec. Licks the pan. Sniffs 'ovals,' but turns away. Then comes back and licks crumbs remaining in the 'sergeant' pan. Turns away from the pans, leaving the 'ovals' untouched.

The following points are significant in connection with the behavior in the sated state. (a) Decrease in hunger results in a decrease in the sort of behavior that occurs in the training situation. This would be consistent with the substitute stimulus conception if the latter behavior were identical with the unconditioned behavior (which it is not). The correlated decrease is, however, consistent with the sign-urge conception which stresses the dependence of goal directed behavior upon the state of the urge system involved. Change in hunger should thus result immediately in a change in behavior without additional training. (b) The decrease of restless movements in moderate satiation and their later increase with more complete satiation (correlated with the appearance of clear *avoidance* reactions to the food itself) are consistent with the sign-urge conception, according to which restless movements result from blocking a tendency toward *or* away from the food. (c) The clearly marked difference in behavior (overt and secretory) to conditioned stimuli signalizing foods which will be eaten, after partial differential satiation, and those which will be refused, is consistent with the sign-urge conception. Since the state of hunger during training was identical for the two stimuli and since both are later tested after the same change in state (*i.e.* same present condition of the stomach without any intervening presentation of the stimulus), this finding is not consistent with the substitute stimulus theory, which would predict a similar change in behavior to both conditioned stimuli. (d) The lack, during satiation, of decrease in conditioned secretion *or* of any marked change in overt behavior to conditioned stimuli reënforced with *acid*, constitutes a striking contrast with the changes resulting in the food behavior. This is completely consistent with the requirements of the sign-urge conception but—for the same reasons as in (c) above—equally inconsistent with the substitute stimulus requirements.

(*2*) *Behavior during experimental extinction.* Further psychologically

significant concomitance of secretory and overt behavior is found in the process of experimental extinction. The behavior of different dogs in the process of extinction varies greatly but it may all be said to fall within the following descriptive groups.

(*a*) *Decrease in positively directed movements.* At first there is an increasing delay of the initial glance into the food-pan; later, hesitation in looking at the conditioned stimulus itself; and finally neither the food-pan nor the conditioned stimulus may be looked at previous to the time at which food should appear. Along with these temporal changes go a decrease in the *precisness of fixation:* the conditioned stimulus may not be looked at directly; the head may be moved half way or only slightly in the direction of the food-pan.

(*b*) *Increase in restless movements.* Stamping and shifting of position may increase. Reactions may be made to objects in the environment which are never responded to under conditions of training. Thus there may appear exploratory sniffing of the chute, table top, or supports over the head. Occasionally barking occurs. The extent to which behavior of (*a*) or (*b*) appears varies greatly and depends upon constitutional factors of temperament and stability rather than upon the circumstances of the training or extinction situation.

(*c*) *Appearance of negative movements.* Appearance of movements directed *away from* the food-pan, particularly after the failure of food to come, at the 15th sec. These movements show a roughly progressive increase in amplitude, vigor, and definiteness as the conditioned secretion decreases and after it disappears. They may consist in pulling back on the chain away from the food-pan, swinging the head to one side to 90° or even 180° and, if freed, finally leaving the stand. The movements of this class may be slight; they may emerge sporadically out of a behavioral picture of increasing apathy; or they may progressively come to dominate the picture, depending on the dog.[30]

Regardless of individual differences, there is always a correlated and roughly progressive change in both secretion and overt behavior. Behavior of types (*b*) and (*c*) has not received sufficient attention before. It is undoubtedly related to the "inhibitory" phenomena of extinction beyond zero emphasized by Pavlov.[31]

The behavioral and secretory changes which occur during the last stages of satiation and of extinction are very similar and point to some similarity in the dynamics of the situations.

(*3*) *Change of position of the animal.* Application of the typical *Umweg* technique to the present behavior would require placing the dog in a position in the otherwise unchanged experimental situation from

[30] Behavior of the types described was shown in a film at the meeting of the American Psychological Association, Hanover, 1936, *Psychol. Bull.*, 33, 1936, 751.

[31] To one who has observed the behavior in extinction and in other conditions the explanation that this type of behavior is the result of random stimuli or of old associations is inadequate. It is clearly directed *away from* the position of the goal objects of the former situation, and demands for its explanation the assumption of an increasing *aversion* to the situation. This assumption is, of course, not demanded by the sign-urge conception, but would be a necessary addition in order to account for the observed behavior.

which he had never before experienced the conditioned stimulus. If an articulated sign organization not too embedded in the total situation had been built up, and if the perceptual organization of the situation were clear enough to allow the actual possible routes of approach to be perceived by the animal as means routes (this can be tested by means of *other* goal objects), then the sign-urge conception would demand that *on the initial trial* the food-pan be approached upon presentation of the conditioned stimulus reënforced with food (avoided, or or at least not approached, with an acid reënforced stimulus). In its simple form the substitute stimulus theory would predict either the occurrence (in whole or in part) of movements made by the animal in the training position; or the appearance of new movements directly related to the new stimuli impinging upon the animal. No definite prediction could be made as to the *direction* of these movements relative to the food-pan without *assuming* some previous connection between food-pan and the responses necessary to reach it from the new position.[32] With regard to more complicated forms of the theory it may be pointed out that the family habit principle is ruled out since the positions during training and test are different.

A large number of experiments of this type have been carried out by the writer. The results are too complicated to be briefly generalized. Both muscularly similar and appropriately varied behavior occurs, depending on the dog and the detailed organization of the situation. Although the kind of adaptive variations that occur strongly indicate (to the writer) the necessity of assuming guidance by a reorganized perceptual system of the type postulated by the sign-urge theory, it is clear that the conception requires further differentiation and specification. Specifically it will be necessary to test not only whether behavior indicates "a" sign-significate organization, but also to determine more precisely the concrete nature of this relationship, its inner articulateness, the degree to which it is functionally independent of or closely embedded in the total situation, its rigidity, degree of mechanization, and other yet more vaguely known features. Further experimentation and theoretical refinement is necessary for the functional definition of these factors and their identification and control in concrete situations.

[32] Although this possibility is ruled out by the conditions of the present experiment, the further assumption is often made that sometime in the past history of the organism a *similar* set of responses had been made to a *similar* situation. Since the necessary degree of similarity is not defined the assumption can not be disproven and the hypothesis remains untestable.

This will be necessary, however, before really precise prediction is possible on any theory.

One set of experiments may be described here, however, in which different *initial* behavior would be predicted by the two major conceptions under consideration. Two new stimuli were reënforced (on the training table, T_1) approximately the same number of times, one with food, the other with acid. The animal was then placed without straps at the back of a second table, T_2 (placed parallel to the first) and from which it had never before experienced either conditioned stimulus. Both conditioned stimuli were then presented to the dog in the new position after each had just been previously reënforced in the training situation. A single experiment in which the *initial* trials in the new position occurred is protocolled below on p. 401.

The same experiment was repeated with three other dogs. Only the initial trials in the new position will be commented upon. With Stella complete approach behavior to the food-pan occurred from two new positions on T_2 when the conditioned food stimulus was given. When the conditioned acid stimulus was presented the dog was lying down on T_2 and failed to rise; yet she jumped up immediately, crossed over, and looked into the food-pan when the conditioned food stimulus was given shortly afterwards (Fig. 5). With the other two dogs no complete approach to, or increased avoidance of the food-pan occurred. The behavior, although grossly similar to that on T_1 was significantly modified. With the conditioned food stimulus the food-pan was fixated *from the new position* (*i.e.* not by identical movements) although it was not approached before the fall of food. The acid behavior was similar to that on T_1 (Fig. 6). With the last two dogs the situation was obscured by the fact that the whole experimental chamber had become negative, making it necessary to scold the dogs sharply to prevent them from leaving T_2 before the conditioned stimuli were presented. This may easily have inhibited any pronounced approach or avoidance behavior.

In summary it may be stressed that although more complete adaptive variation in behavior might have been expected, yet *all* the new behavior that did occur in all the dogs is consistent with the sign-urge conception. Tests of conditioned secretion made later with the dogs restrained with straps on T_2 showed that the drop in secretion from the training situation was about the same for both food and acid. This illustrates strikingly the fact that although an observationally isolated component of two responses may be similar, or change similarly in a changed situation, the complete behavior may be strikingly different, especially in its directional and energetic aspects.

Taken all together, the behavior elicited under the various changes in condition suggest that the entire conception implied by the term conditioned *response* is at fault. It suggests strongly that "a" response, even a complex one, is not conditioned at all; that the primary structural change is not a connection between the conditioned stim-

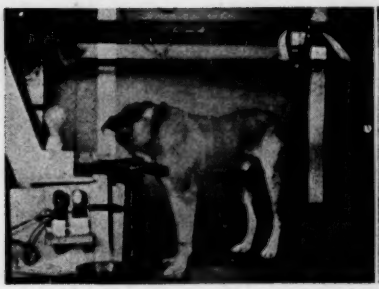

FIG. 1. BOB: TRIAL 3
(Response to pipe = conditioned acid stimulus.)

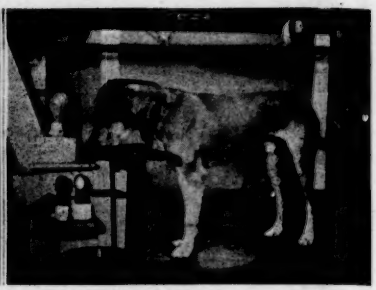

FIG. 2. BOB: TRIAL 4
(Looking into food-pan after crossing from T_3; moving card = conditioned food stimulus.)

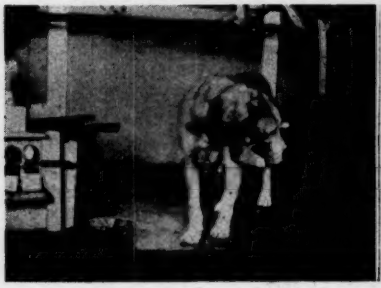

FIG. 3. BOB: TRIAL 6
(Starting to turn away from food-pan and to walk off table; pipe = conditioned acid stimulus.)

FIG. 4. BOB: TRIAL 9
(Starting to turn away from food-pan and to jump behind T_1; pipe = conditioned acid stimulus.)

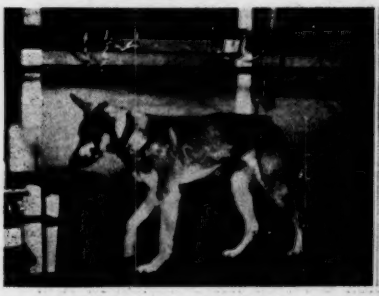

FIG. 5. STELLA LOOKING INTO FOOD-PAN AFTER CROSSING FROM TABLE 2.
(Moving card = conditioned food stimulus)

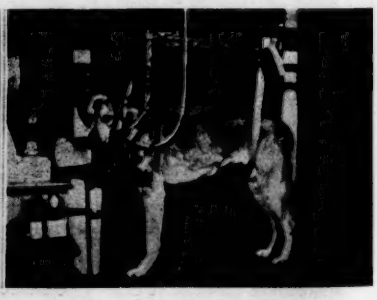

FIG. 6. STELLA LOOKING AWAY FROM FOOD-PAN AND TELEGRAPH CLICKER
(Same day; telegraph clicker = conditioned acid stimulus.)

CHANGE OF POSITION: INITIAL TRIALS[38] (Bob; Feb. 15, 1937; filmed)

Trial 1. Moving card (food). T_1: training position; strapped. Holds head over food-pan with several slight cocking movements, gradually lowering it into pan. (C.S.=23.8 cm.) Eats.

Trial 2. Moving card (food). T_1: unstrapped. Head on shelf. Raised head at movement of card; held over food-pan, gradually sinking into it. No change in bodily orientation. Eats.

Trial 3. Pipe (acid). T_1: unstrapped, acid tube not connected. Position as before. At pipe raises head high, then to right and down, licking chops (Fig. 1). Then moves head across food-pan ninety degrees to left, then again to right and down. After cessation of pipe rests head on shelf. No acid given.

Trial 4. Moving card (food). T_2: in rear; free (*initial trial in this position*). Experimenter has to shout to prevent animal from leaving T_2. About 30 sec. after experimenter leaves the room the card is started moving. Bob looks at it, and without hesitation jumps across to T_1 and stands looking directly into the food-pan until the food falls (Fig. 2). Eats.

Trial 5. Pipe (acid). T_1: strapped and acid tube connected. Behavior similar to trial 3. (C.S.=7.8 cm.) Reënforced with acid.

Trial 6. Pipe (acid). T_2, in rear; free (*the pipe has not previously been experienced from this position*). After experimenter leaves Bob crosses to T_1; the second time Bob walks down the stairs to the door. Third time he is restrained on T_2 by shouting. Remains after experimenter leaves, standing facing T_1 with head depressed. He looks toward the pipe when it sounds, then jumps across to T_1. Standing cross-wise of T_1 he looks toward the back of the table, then forward toward shelf at edge of food-pan, then toward back of table. At about 10th sec. he slowly turns (see Fig. 3), walks to back of table and down the stairs to floor before the 15th sec. (at which acid would normally be given).

Trial 7. Bell (food). T_2, in rear; free (*this stimulus has previously not been presented in this position*). Gives bell brief glance and crosses immediately to look into food-pan. Oscillates between bell and food-pan. Eats.

Trial 8. Pipe (acid). T_1; strapped; acid tube attached. At pipe swings to right with head down; turns left when acid comes. (C.S.=11.6 cm.)

Trial 9. Pipe (acid). T_2, in rear; free. Is facing T_1 as in Trials 4, 6, 7. Glances at pipe, then slightly to left. Turns toward rear (away from the food-pan; see Fig. 4) and jumps off of the stairs behind T_1.

ulus and a definite conditioned response; but rather reorganization of a perceptual system related to an urge system whose combined action releases (not a single definitely defined response) but behavior characterized primarily by its specific direction relative to the organization of the goal, means, and barriers of the given situation. The conditioned response resembles the unconditioned as far as it does only because this organization is usually kept identical in training and test situations. From this point of view the designation of this type of learning by the term conditioned *response* is theoretically misleading. A new terminology is desirable.

SUMMARY

(1) A more complete description of the overt behavior occurring during conditioned salivation had been presented. Differential implications of two conceptions of the conditioned response have been

[38] The stimulus-card, shown in Figs. 1-6, moved up and down about once a second; the pipe was located just in front of the animal to the right of the food chute. Bob received acid in the right cheek, Stella in the left.

tested by application to this behavior. The behavior studied was elicited both in situations identical with that during training and varied from it in certain ways. These variations were produced by decreasing hunger, bringing about extinction, and changing the position of the animal within the situation.

(2) The constant occurrence of biologically significant co-variation of the secretory and overt components of behavior is considered incompatible with a response element theory of conditioning such as that of Pavlov.

(3) The lack of identity in effector terms of conditioned and unconditioned behavior even in the training situation, but especially when the situation is varied, is considered to be incompatible with all stimulus substitute theories of conditioning. Assertions of identity of behavior can only be maintained if observation is restricted to the secretory component. This can only be theoretically defended if one holds to the conception of independent response elements controverted by the facts referred to in (2). The conditioned salivary response is thus to be added to those other responses to which the substitute stimulus conception has been shown to be inapplicable.

(4) Conditioned salivation is representative of the simpler less voluntary, more 'reflex' types of conditioned response. The basic conditioned response generalizations of Pavlov were derived from its study. Its apparent simplicity is here shown to result from experimental restriction of observation to a single component of behavior; from restriction of the possibilities of behavior by physical and psychological barriers; from failure to study the resulting structural reorganization by systematic variation of the training situation; more indirectly, from the theory of response elements. If, as in the present study, the limitations of these conditions are partially removed, the salivation reveals itself as a dependent component within a complex, widespread, organized, goal-directed behavioral act, which varies adaptively with changes in the experimental situation. This strongly suggests that the same may prove to be true of many if not all of the apparently simple responses which have been conditioned with apparent identity of conditioned and unconditioned response; and that some appropriate variation of behavior of the type here demonstrated may be a *universal* characteristic of conditioning. In this case the basic problem should not be stated: does variation occur; but rather what sort of variation occurs and upon what conditions does it depend.

In any event it is felt that a more careful examination, experimental and theoretical, of the factual basis of the fundamental *postulates* of current associationistic conditioning theories would be a wise supplement to current elaborate deductive explanation of other types of behavior.

(5) It is felt that the data here presented require a more complex conception of the structural change produced by conditioning. A sign-urge conception is outlined in which both reorganization of a perceptual system and its relation to an urge or tension system are essential features. It is maintained that this conception possesses sufficient predictive precision to enable it to be tested and thus further refined.

(6) The fruitfulness of the method of testing conditioned response behavior under conditions varied systematically from those of training is felt to have been demonstrated; as well as the desirability of observing more completely the total behavior occurring, especially its directional features.

DAS PROBLEM DER INDIVIDUALITÄT

By Tanenari Chiba, Tohoku Imperial University
Editor, *Tohoku Psychologica Folia*
(Founded in 1933)

(1) Es entstehen unendlich verschiedene Variationen der Individualität dadurch, dass es genetisch verschiedene Abzweigungen und differentiell verschiedene Verteilungen gibt. "Man hat seine eigene Natur, wie sein eigenes Gesicht." "Zu zehn Männern, zehn Farben." Nein, es gibt so viele Verschiedenheiten der Individualität wie die Zahl der Menschen. Es gibt für jedes Individuum; nicht nur für die menschliche Seele, sondern auch für den menschlichen Körper, ebenso für andere Lebewesen, d.h. für alle Tiere und Pflanzen und für alle Gewebe und Zellen. Es scheint auch für Elemente, Atome und Elektronen gültig zu sein. Nicht nur im natürlichen, sondern auch im künstlichen Gebiete ist Jedes verschieden, wenn ein Einziges geformt wird. Wenn wir für alle zusammengehörigen individuellen Charaktere die Bezeichnung "Individual-Qualität" wählen, so können wir sagen: an jedem Individuum erscheint seine eigene "Individual-Qualität."

(2) Was solche Verschiedenheit ist und warum sie entsteht, lasse ich hier beiseite. Ich werde jetzt nur untersuchen, wie diese Verschiedenheit erscheint. Wir können auf jeden Fall sagen, dass es spezifische Eigenschaften gibt. Bei einer Person sehen wir Schärfe und Stumpfheit der Sinnlichkeit, Frische und Dunkelheit der Vorstellungsbilder, Klarheit und Unklarheit der Urteile, Stärke und Schwäche der Gefühle, Reichtum und Ärmlichkeit der Affekte, Schnelligkeit und Langsamkeit der Reaktionen, Dauerhaftigkeit und Veränderlichkeit des Willens, etc., wie Einfachheit und Zusammengesetztheit, Grossheit und Kleinheit der Blumenblätter, Dunkelheit und Helligkeit der Farben, Härte und Weichheit der Qualität, lange und kurze Lebensdauer bei einer Rose. Die Individualitäten sind also mannigfache Zusammensetzungen unendlicher Gegensätze dieser verschiedenen Eigenschaften:

$$x_1 a_1 + y_1 b_1$$
$$+ x_2 a_2 + y_2 b_2$$
$$+ x_3 a_3 + y_3 b_3.$$

Dieser Gegensatz ist aber nicht absolut. Wenn ein Urteil auch noch so klar ist, so ist es in einem Individuum doch mehr oder weniger unklar. Die Dauerhaftigkeit des Willens steht im Vergleich zur Veränderlichkeit. Es existiert weder absoluter Fleiss noch absolute Faulheit. Es scheint beides im Widerspruche zu stehen, aber es sind nur zwei relative Pole der Tätigkeit, und zwischen diesen Polen bestehen verschiedene Gradunterschiede. Wenn wir an den Fleiss denken, setzen wir dabei die Faulheit voraus; wenn wir an die Faulheit denken, setzen wir schon den Fleiss voraus. In diesem Sinne ergänzen sich beide. Wir können aber durch alle diese Gegensätze den Gegensatz zweier Grundeigenschaften vorstellen.

Hier kann man leicht erkennen, dass die Denkweise von "Eki" uns vieles lehrt. Sie stellt zwei Prinzipien auf: "In" (=Negativität) und "Yô" (Positivität), die in allen gegensätzlichen "Erscheinungen" z.B.: nieder und höher, edel und gemein, bewegt und ruhig, hart und weich, Sonne und Mond, kalt und warm, krumm und gerade, etc., herrschen würden. Diese zwei Prinzipien sind, wie Tanabe feststellte, ein ideelles Gegenüberstehen.[1] Es existiert keine Ruhe ohne Beweglichkeit, ebenso wie keine Beweglichkeit ohne Ruhe; die Beweglichkeit wird Beweglichkeit erst mit Berücksichtigung der Ruhe, und auch die Ruhe mit der der Beweglichkeit. "Taikyoku" (der äusserste Höhepunkt) ist keine Beweglichkeit und keine Ruhe, aber er ist sowohl reine Beweglichkeit als reine Ruhe. In diesem ideellen Gegenüberstehen vermittelt das eine das Bestehen des anderen, obwohl jene im Gegensatz zu diesem steht. Beide gehören zu einem System, welches aus beiden entsteht, ohne dass das eine das andere erobert. Die Charakteristik des ideellen Gegenüberstehens betrifft einen Gegensatz innerhalb zusammengehöriger Eigenschaften. Der Gegensatz von "In" und "Yô" ist nichts anders als ein solches Gegenüberstehen.

Diese Betrachtungsweise wird auch für das Gegenüberstehen der Eigenschaften einer Individualität gültig sein. Der obige Gegensatz der verschiedenen Eigenschaften einer Individualität ist schliesslich als der Gegensatz von zwei Grundeigenschaften aufzufassen. Wenn wir diese als "Yôsei" (positive Eigenschaft) und als "Insei" (negative Eigenschaft) bezeichnen können, so lässt sich denken, dass die unendlich vielen gegensätzlichen Eigenschaften den obigen Gegensatz einschliessen werden. Beides steht nicht im absoluten Gegensatz; der eine vermittelt vielmehr das Bestehen des andern, obwohl jener im Gegensatz zu diesem steht; beide gehören zu einem System, zu einer Individ-

[1] G. Tanabe, Über die "Juchiaosche" Ontologie, *Festschrift Dr. Takase*, 1928, 13.

ualität, ohne dass der eine den andern erobert. Und es ist klar, dass es ein relativer Gegensatz ist, der zu einer Ordnung gehört. Wenn wir es so verstehen können, dann können wir irgend eine Individualität wie unten bezeichnen.

$$\begin{aligned} K &= XO + YI \\ &= x_1 o_1 + y_1 i_1 \\ &+ x_2 o_2 + y_2 i_2 \\ &+ x_3 o_3 + y_3 i_3. \end{aligned}$$

Diese Beziehung können wir schematisch folgendermassen darstellen. Weiss und Schwarz bezeichnet "Yôsei" und "Insei." Je mehr nach rechts, desto mehr nimmt die "Yôsei" Qualität ab, und die "Insei" Qualität zu. So entstehen unendlich verschiedene Individualitäten durch die Zusammenstellung von zwei Grundeigenschaften. Je näher dem Pole, desto mehr bekommt "Yôsei" oder "Insei" einen besonderen Charakter. Grosse Männer, besonders geniale Menschen, haben solche Individualitäten, obwohl das umgekehrte Verhältniss nicht immer zutrifft. Die Individuen, die in der Nähe der Mitte liegen, zeigen sich als widerspruchsvolle Charaktere, gehören manchmal zu den ungewöhnlichen Menschen. "Zwei Seele wohnen, ach! in meiner Brust;" das gilt zwar in irgend einem Sinne für jedes Individuum, aber besonders auffallend ist es bei obigen Individuen. Die Durchschnittsmenschen gehören zu den drei oben bezeichneten Gebieten oder zu dem dazwischen eingeschalteten, weiten Gebiete.

Wir können also sagen, dass die Individualität aus der Verästelung und der Verteilung der zwei Grundeigenschaften, "Insei" und "Yôsei" geformt ist, wobei diese als die Differenzierung von etwas ursprünglich Einheitlichem anzusehen sind, oder so, dass in einer Individualität zwei Eigenschaften, "Insei" und "Yôsei," zu denken sind, die als ein relativer Gegensatz unter dem einheitlichen Eins enthalten sind.

(3) Wie wir schon gesehen haben, ist die Individualität die dem Individuum eigene Besonderheit. Sie ist eigentlich angeboren, "arbeitet immer eigenbewusst.[2] Die Individualität ist also ursprünglich und von äusserlichen Einflüssen nicht ableitbar. Sie ist jedem Individuum zueigen, und hier wird das Problem der Disposition von Bedeutung. Wie Witasek sagt: im psychischen Leben gibt es "eine grosse Menge von Einzelerfahrungen, die sich nur unter dem dispositionstheoretischen Gesichtspunkte in allgemeine Gesetze fassen lassen, das heisst also, nur dann, wenn man sie in ihrer Beziehung zu den Dispositions-

[2] T. Chiba, Der Wille als "Eigenbewusstsein," *Tohuku Psychol. Folia*, 2, 1935, 129-144.

grundlagen, als deren Leistungen sie anzusehen sind, betrachtet." Es ist unrecht, dass wir, wie Herbart, eine unbewusste Vorstellung annehmen, welche unter der Schwelle fortgeht. Aber wir können auch nicht wie Witasek denken, dass das Residuum die Fähigkeit hat, die Vorstellungen wieder hervorzurufen, d.h. die wirklichen Vorstellungsgrundlage zu erzeugen. Wir müssen hier die grosse Tathandlung des Eigenbewusstseins, zu dem das gewöhnlche Bewusstsein gehört, annehmen. Die Wirkungen des Eigenbewusstseins verzweigen und differenzieren sich durch irgendeine Hemmung, und dadurch entsteht der Gegensatz von "Insei" und "Yôsei," worin die Indiviualität besteht. Aber wir dürfen nicht vergessen, dass dieser Gegensatz eigentlich das Gegenüberstehen der ideellen, relativen Gegensätzlichkeit als Differenzierung des einheitlichen Eins ist, und dass er also ein Gegensatz in dem Sinne ist, dass das eine jeweils das andere voraussetzt.

Gemäss diesem Gedanken kann man unendlich viele Gegensätze von Eigenschaften, in Bezug auf die Erscheinungsweisen der Individualität, vorstellen. Es gibt aber auch vielleicht zu Grunde liegende Hauptgegensätze. Der eine von diesen wichtigen Gegensätzen bezieht sich auf "Eigenwerden."[3] Von den verschiedenen Beziehungen des "Eigenwerdens" aus entsteht die Nuance-Verschiedenheit der Individualitäten. Die verschiedenen Gegensätze der Individualitäten erscheinen so als das Ergebnis verschiedener Eigenschaften und auch verschiedener Gesichstpunkte. Jedenfalls scheint es sicher zu sein, dass die verschiedenen Beziehungen des "Eigenwerdens" eine der wichtigsten Bestimmtheiten der Individualität sind.

Es macht vor allem einen Gegensatz, je nach dem ein "Eigenwerden" vorherrscht oder nicht. Es gibt z.B. ein Individuum, in dem Gedanken und Gefühle vom Anfang an klar sind und ohne weiteres klar erscheinen. Doch gibt es auch anderseits Individuen, in denen sie immer wie durch den Nebel gesehen und daneben viele andere unklare Gedanken und Gefühle erlebt werden, sodass sie im Ganzen sehr düster, trübe, und matt sind. Solche Tatsachen können wir täglich vor uns sehen. Wir können auch beobachten, dass infolfge der Verschiedenheit der Tiefe, der Breite oder der Stärke und der Dauer verschiedene Individualitäten oder charakteristische Persönlichkeiten entstehen. Dies ist vielleicht der ursprünglichste Grund der Klassifikation der Individualitäten.

(4) Wenn wir die Erscheinungsweisen der Individualität unter

[3] T. Chiba, Über das "Eigenbewusstsein," *Jap. J. Psychol.*, 2, 1927, (no. 5), 1-6.

obigem Gesichtspunkte sehen, so ergeben sich daraus einerseits die Verschiedenheiten des Talents, des Temperaments oder des Charakters und anderseits die Einteilung der Typen. In jenen sieht man die verschiedenen Eigentümlichkeiten von Richtungen der Seelentätigkeit und in dieser, dass es je nach den eigenartigen Zügen als Ganzes mehrere Typen gibt. Nun sehen wir das letzte Schema: wenn wir auf die beiden Pole oder ihre nähere Umgebung hinsehen, so können wir zwei oder vier Typen unterscheiden; wenn wir auch noch die Mitte in Betracht nehmen, so entstehen drei oder fünf Typen; und wenn wir weiter die dazwischen liegenden Regionen betrachten, dann können wir fünf oder sieben Typen unterscheiden. Dies ist der Grund, warum bisher so verschiedene Klassifikationen der Typen aufgestellt worden sind.

(5) Wir betrachten nun von unserem Gesichtspunkt aus die bisherigen Theorien über die Individualitätsverschiedenheit. Hippokrates-Galenus' vier Temperamente wurden auf die Mischung der verschiedenen Säfte im Körper: des Blutes (Sanguiniker), der gelben Galle (Choleriker), der schwarzen Galle (Melancholiker) und des Schleims (Phlegmatiker) zurückgeführt. Man nahm an, dass die Gemütsart von dem Vorwiegen eines dieser Säfte herrühre. Es gibt dann zwei Gegensätze von Eigenschaften: Wärme und Kälte, Trockenheit und Feuchtigkeit, je nach dem Temperaturgrad und Feuchtigkeitsgrad. Die neueren Darstellungen führen meist auf Kants Charakteristik der Temperamente in seiner Anthroplogie zurück. Kant unterscheidet Temperamente des Gefühls und Temperamente der Tätigkeit. Temperamente des Gefühls sind: das "sanguinische Temperament des Leichtblütigen" und das "melancholische Temperament des Schwerblütigen." Temperamente der Tätigkeit sind: das "cholerische Temperament des Warmblütigen" und "das phlegmatische Temperament des Kaltblütigen." Innerhalb des Gefühlsverlaufs selbst begegnet uns zunächst der Gegensatz der Stärke, Schwäche und dann der Dauer und des Wechsels der Gefühle. Beide Gegensätze kreuzen sich, so dass wir vier Arten bekommen, eine Gliederung der Temperamente, die zuerst Wundt gegeben hat. Elsenhans nahm ausser der Form des Gefühlsverlaufs (Beweglichkeit und Stärkegrad) noch die Erregbarkeit und die Motivationskraft der Gefühle als die Merkmale der Temperaments an und erklärte die vier Temperamente daraus. Heymans wählte zur Gewinnung von Typen drei Eigenschaften: die Emotionalität (E), Aktivität (A), und Sekundärfunktion (S resp. das Gegenteil, die Primärfunktion P), weil diese in der klassischen Temperamentslehre zweifellos eine hervorragende Rolle spielen. Durch Kombination derselben oder ihres

Gegensatzes ergeben sich dann acht Temperamente. Nach der Grösse der Emotionalität oder der Aktivität ergeben sich vier Temperamente und jedes Temperamente teilt sich je nach den Funktionen primär oder sekundär, das macht also acht Temperamente.

Die obigen Klassifikationen sind schliesslich alle auf die bisherige Theorie von vier Temperamenten gegründet. Sie bestimmen sich aus der Vereinigung von Grösse der Erregbarkeit der Gefühle und Grösse der Aktivität des Willens. Dies mache in Wirklichkeit die Grundlagen der Individualität oder des Charakters aus. Diese Klassifikationen sind heute noch sehr verbreitet, obwohl es dabei etwas unklare Punkte gibt.

Wenn wir diese Klassifikation auf das oben gezeichnete Schema anwenden, dann können wir uns vorstellen, dass der Cholerische und der Melancholische an den beiden Polen, der Sanguinische einerseits und der Satiriker, das Spitzbübchen oder der Kritiker andererseits die einander Näherstehenden sind. Der Phlegmatiker liegt dazwischen. In Wirklichkeit haben aber der Choleriker und der Satiriker viele Ähnlichkeiten, und der Unterschied hängt nur von der Dauerhaftigkeit ihrer Seelentätigkeit ab.

Nach unserer Überlegung können wir schliesslich, wie schon oben erörtert, zwei gegensätzliche Eigenschaften, "Yôsei" und "Insei," unterscheiden. Wenn wir also ohne künstliche Bearbeitung schlicht denken, dann wird es richtig sein die auf diesen Gegensatz begründete Klassifikation vorzunehmen. Es ist sehr richtig, dass sich Jungs Theorie, welche nach der Wirkungsrichtung der seelischen Energie zwei Typen, "Extraversion" und "Introversion," unterscheidet, neuerdings sehr verbreitet und auf verschiedenen Gebieten grossen Einfluss ausgeübt hat. Nach dem Weltkriege sind ähnliche Klassifikationen des Charakters massenweise hervorgekommen. Es gibt viele ähnliche Klassifikationen: z. B. in "männliches" und "weibliches" Geschlecht von Weininger, in "intuitive" und "craft mind" von Goldenweiser, in "introversive" und "extratensive types" von Rorschach und "reactive stable" und "reactive labile" von Luria, etc. Besonders ist Kretschmers Unterscheidung, von "zyklothym" und "schizothym," welche auf psychiatrischen Tatsachen fusst, bemerkenswert. Diese Theorien sind sehr geschätzt und haben eine Menge Anhänger bekommen.

Im allgemeinen können wir also sagen, dass die bisherigen Theorien aus der verschiedenen Kombination von zwei Grundeigenschaften vier oder acht, oder auch zwei Temperamente oder Charaktere zu unterscheiden versuchten. Dies ist von unserer obigen Betrachtung aus als richtig anzuerkennen. Es wird im Grunde nur ein Gegensatz gedacht,

obwohl es dabei unendlich verschiedene Eigenschaftsgegensätze gibt. Und zwar können wir uns unter ihnen noch einige Hauptgegensätze vorstellen. Vorläufig können wir die Gefühlserregbarkeit und die Willenstätigkeit unterscheiden und vielleicht noch das "Eigenwerden" des Bewusstseins hinzufügen. Dabei müssen wir beachten, dass es keine richtige Ansicht ist zu meinen, dass alle Individuen irgendeiner Eigenschaft gehören. Wie wir schon oben sahen, sind die bedeutenden Charaktere nur in der Nähe der beiden Pole oder dazwischen gelegen. Die meisten andern Individuen gehören mehr zum Mitteltypus. Es ist ganz unsinnig, alle Individuen irgendeinem Typus zuzuordenen. Für den Individualität- oder Charakter-Analytiker ist solche Überlegung besonders wichtig.

(6) Wenn wir nun unter Berücksichtigung der obigen Gesichtspunkte die Ergebnisse der künstlichen Beobachtungen—experimentellen Untersuchnugen—betrachten, können wir bemerken, dass es viele, die obige Ansicht bestätigende Tatsachen gibt. Hier wollen wir uns nur auf einige wenige Beispiele beschränken.

Zunächst ist die Funktion der Anschauungsbilder heute noch unklar, aber an ihrer Tatsache kann man nicht zweifeln. Dabei sind aber der eidetische und der nicht-eidetische zwei Pole, und zwar sind sie auf wenige Personen beschränkt. Die Majorität liegt dazwischen, sie gehört zum Mitteltypus und lässt sich nach dem Grad verschiedener Abstufungen unterscheiden.[4] Bei dem Icherlebnis oder dem autoskopischen Erlebnis sind je nach dem, ob das Ich klar erscheint oder nicht, zwei Pole gegeben. Solche Individuen sind aber nur auf einen Teil beschränkt, die meisten anderen gehören zum Mitteltypus, und in der Nähe der beiden Pole lassen sich zwei weitere Typen unterscheiden. Also kann man hier fünf Abstufungen unterscheiden.[5] Ausserdem haben dies noch viele andere Untersuchungen, z.B. des Zeitbewusstseins, der Tonempfindungen, der Reproduktionen und der Phantasie-Transformation klar bestätigt.[6]

Diese Verhältnisse können wir bei der Untersuchung der Beziehung der Temperamente mit den Bluttypen besonders klar beobachten. Seit

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[4] Yosikazu Ohwaki, Tosimitu Kaiwa, und Katumi Kaketa, Psychologisch-medizinische Untersuchung der eidetischen Anlage japanischer Jugendlicher, *Tohoku Psychol. Folia*, 2, 1934, 57-128; 4, 1936, 17-50.

[5] H. Kitamura, *Über das autoskopische Erlebnis.*

[6] Saburô Abe, Neue Untersuchung über die absoluten Eindrücke im Gebiete der wahrnehmbaren Zeit, *Tohoku Psychol. Folia*, 2, 1935, 145-180; Kiyosi Takano, Contrast of sound, *ibid.*, 1, 1933, 53-70; 2, 1934, 1-14; Tukasa Susukita, Über das Gedächtnis für lust- und unlustbetonte Erlebnisse im Alltagsleben, *ibid.*, 2, 1934, 43-56; 3, 1935, 187-204; Mansyû Abe, Über die Transformation des Bewusstseins, *ibid.*, 3, 1936, 169-186.

Furukawa dieses Problem erstmals herausgestellt hat, hat man es sehr oft diskutiert, und die Ergebnisse der Untersuchungen sind auch sehr verschieden.[7] Heute kann man noch keine genaue Theorie feststellen. Nach unserer Erfahrung, die wir bisher prüften, können wir wenigstens folgendes als sicher annehmen:

(*a*) Es gibt zwei Arten von Menschen, d.h. von Charakteren des Temperamentstypus: auffallende und nichtauffallende; die meisten fallen dazwischen und gehören zum Mitteltypus oder zum schwankenden Typus.

(*b*) Die bisheringen Prüfungen des Bluttypus sind meistens nicht genau. Die Untersuchungen sollen von geschickten Prüfern und das Verfahren nicht mit der Speichelflüssigkeit, sondern möglichst mit dem Blute durchgeführt werden. Wenn wir nun unter Berücksichtigung dieser Umstände die Ergebnisse betrachten, dann müssen wir anerkennen, dass es eine eindeutige Beziehung zwischen dem auffallenden Temperamentstypus und den Bluttypen gibt.

Diese Verhältnisse können wir in obiges Schema einordnen. Dann sind der O-Typus und A-Typus zwei Pole und daneben liegt der B-Typus einerseits und der Satiriker und Kritiker andrerseits; der AB-Typus liegt dazwischen. Es ist demnach richtig, wenn die meisten statistischen Ergebnisse sehr verschieden sind, in welchen die wenigen auffallenden und die zahlreicheren nichtauffallenden gemischt berechnet sind.

Das gilt auch für die Intelligenzprüfung. Bei der Schulprüfung könne wir den Vorzüglichen und den Minderwertigen sogleich bemerken. Die meisten fallen dazwischen. Konfuzius sagt: "Bloss den Vorzüglichen und den Minderwertigen kann man nicht verändern." Bei der Intelligenzprüfung kann man den Vorzüglichen und den Minderwertigen von den andern leicht und klar unterscheiden und die meisten andern fallen dazwischen. Es ist also sehr fragwürdig, wenn man diese Verschiedenheiten statistisch gleich behandelt und den Durchschnitt zieht.

(7) Wir haben zuerst die Begriffe der Individualität und die darauf bezüglichen Gedanken überblickt und gefunden, dass die Individualität eine Einheit der Eigenschaften ist, welche das Individuum charakterisieren. Auch haben wir geschildert, dass sie mit dem Temperament oder dem Charakter verwechselt wird, weil die Grundeigenschaft auf

[7] Takeji Furukawa, Die Erforschung der Temperamente mittels der experimentellen Blutgruppenuntersuchung, *Ztsch. f. angew. Psychol.*, 31, 1928, 221-299.

den Gefühlen und der Willensanlage beruht. Nach der gründlichen Betrachtung der Individualität haben wir dann erklärt, dass es genetisch je nach der verschiendenen Verzweigung einerseits und differentiell nach der verschiedenen Verteilung andererseits die unendlich verschiedenen Formen der Individualitäten entstehen. Was die Struktur der Individualität betrifft, so haben wir gesehen, dass es in jeder Individualität einen Gegensatz von spezifischen Eigenschaften gibt. In diesem Sinne sind die Individualitäten verschiedene Gebilde der unendlich verschiedenen Gegensätze von diesen verschiedenen Eigenschaften. Der Gegensatz ist aber nicht ein absoluter, sondern ein idealistischer, in welchem das eine immer das andere voraussetzt. Die verschiedenen Gegensätze der Eigenschaften innerhalb der Individualität gehen zurück auf den einen Gegensatz der zwei Grundeigenschaften: "Yôsei" (O) und "Insei" (I). Kurz, man kann sagen, dass die Individualität aus der verschiedenen Zusammenstellung besteht. Mit anderen Worten, die Individualität ist aus der Verzweigung und Verteilung der zwei Grundeigenschaften "Yôsei" und "Insei" (worin alle einzelnen Eigenschaftsgegensätze eingeschlossen werden) gebildet. Aber der Individualität ist eigentlich nichts hinzufügen noch ist sie von aussen beeinflussbar, sie ist die jedem a priori zu eigene Eigentümlichkeit. Hier wird das Problem der Anlage wichtig. Die dem Eigenbewusstsein eigenen Wirkungen verzweigen und differenzieren sich durch Hemmung und es entsteht dann irgendein Gegensatz. Auf solche Weise ist die Individualität geformt. Hier kann man unendlch viele Gegensätze der Eigenschaften vorstellen. Aber es gibt doch einige besonders wichtige Gegensätze, welche die Grundlage ausmachen. Einer dieser Gegensätze ist das "Eigenwerden." Wenn man nach obigem Gesichtspunkt die bisherigen Betrachtungen prüft, kann man sich vorstellen, dass man alles als eine Kombination von zwei Grundeigenschaften verstehen kann. Die Gefühlserregbarkeit und die Willenstätigkeit sind die wichtigsten. Es ist also richtig, wenn die moderne Klassifikation der Individualität zur Zweiteilung neigt. Das ist auch von der künstlichen (experimentellen) Beobachtung gültig. Ausser den auffallenden Polen kann man noch zwei oder vier oder noch mehrere nichtauffallende in mehreren Abstufungen abteilen. Es ist also nicht richtig, dass man in den verschiedenen Ergebnissen keine Übereinstimmung finden kann.

Was das Problem der Anlage betrifft, so bleibt noch eine wichtige Frage. Es gab bisher zwei entgegengesetzte Ansichten. Lao Tse verkündete *Mui* = "Nichts tun" und Konfuzius sagte: "Natur ist ähnlich, Gewohnheit ist verschieden." Menzius lehrte *Seizen* = "das Gute der

Natur," und Hsunzius behauptete *Seiaku* = "das Böse der Natur." Die modernen Vertreter dieser entgegengesetzten Ansichten sind Rousseau und Pestalozzi. Rousseau behauptet bekanntlich in dem ersten Satz des Émile "alles ist gut, wie es aus den Händen des Schöfers hervorgeht, alles entartet unter den Händen des Menschen." Pestalozzi nimmt dagegen an, der Mensch neige von Natur zur Entartung und Verwilderung, der Sohn der Freiheit und der König des Raubes ist der Gesellschaft nicht nur nichts nütze, sondern ist ihr im höchsten Grade gefährlich und unerträglich. Nach unserer Meinung sieht jeder bloss eine Seite des Ganzen. In dem eigentlichen Samen ist alles, was sich später daraus entfalten soll, schon entschieden. Es darf aber nicht der Natur überlassen bleiben. Es braucht eine geeignete Leitung, um die richtige Entwicklung erreichen zu können. Aber die sogenannte Leitung kann nicht das eigentlich entschiedene Wesen verändern. Ein Eichen-Same wird mittels Licht, Wasser und Erde zum grossen Eichen-Baum werden; doch kann man ihn niemals zum Fichten-Baum werden lassen.

TIME AND REACTION

By Jacques van Essen, Amsterdam
Editor, *Nederlandsch Tijdschrift voor Psychologie*
(Founded in 1933)

What is time and what is reaction? We shall never know exactly, for both are attributes of our existence, and we should have to step out of that existence to get a really objective view of them. One thing is certain, that reaction takes place in time, and that, consequently, there is a close relation between the two: reaction can be measured by time. And this has frequently been done. Modern psychophysiology partly owes its origin and progress to this.

It is, moreover, also certain that the reverse is possible; namely, the measuring of time by reaction. This is undoubtedly an unusual and, at first sight even a remarkable procedure, but we shall endeavor to show that it is not by any means devoid of sense and even that it is necessary if one wishes to penetrate more deeply into the problem of time and reaction than has hitherto proved possible.

The most obvious measurement of reaction by time, and indeed the most prominent, concerns the so-called *reaction-time*, the time that elapses between the moment at which a signal is given and that at which the reactive movement commences. Measurement soon showed that the length of this interval depends upon the most varying external, or objective, circumstances. Thus, in the first place, it is not a matter of indifference with which sense-organ the signal is observed nor with which motor organ reaction takes place. It was, for instance, very soon discovered that the visual reaction-time is generally longer than the auditory, and that rapid movement is promoted by the natural use of practiced motor organs which are preëminently governed by the will, such as the hand and the vocal apparatus. The strength of the impression made by the signal is also of influence. In order to effect a short reaction-time, the signal must come strongly to the fore, yet it may be too strong as well as too weak. In this respect a very great number of experiments have already been made during the past years. We may also think here of the phenomenon of optimal duration, that is, the most favorable duration producing prompt response in a given

situation. Many experimenters have intensively occupied themselves with the influence of the meaning or the thing-character of the signals. Only think of the many accurate experiments regarding reaction to objects and to words and the choice-experiments connected with them. They found their practical application especially in the experimental psychology of thought and in the psychiatrical clinic.

It is remarkable, however, that people have hitherto scarcely concerned themselves with the certainly no less important fact that the manner in which the signal appears is of great importance. Thus, when measuring both the visual and the auditory reaction-time, we found considerable disparity when the signal consisted of an appearing or of a disappearing impression, and also when the change in the sensory field was of a permanent or of a merely transitory nature. Table I affords an example of this.

TABLE I

RELATION BETWEEN IMPRESSION AND REACTION

(p = ca. 20% of t)

Impression	Reaction-time (run-time)
comes and stays	t + 2p
comes and disappears	t + p
passes	t

From Table I it appears that the reaction-time is greatest in the case of a coming and permanent impression and smallest in the case of a transitory impression. We think it possible to explain this by saying that in the first case the sensorimotor situation changes towards a continuing positive stimulus, which demands a thorough sensorimotor adjustment all along the line and necessarily involves an extra amount of physiological time. In the case of the disappearing and permanently absent impression, the sensorimotor situation also undergoes a thorough change, but towards the withdrawal of stimulation, so that the sensorimotor position with regard to what is happening need not change so radically and the reaction, therefore, costs a little less physiological time. Finally, in the case of the exclusively passing impression, whether coming or going, the call upon the sensorimotor system is smallest because the final sensory situation is the same as at first, so that the sensorimotor system can react more immediately, because partially.

The facts presented in Table I refer to visual and auditory stimuli which were but *insignificant* changes in the sensory field, possessing no real meaning or thing-character whatever. Thus, the visual signal consisted in purely homogeneous (that is to say, a bulky color) illumin-

ation of the previously dark field of vision. For this we used the light-spectacles described by us in a recent article.[1] The auditory signal consisted in the beginning, ending, or transitorily sounding of an amorphous and unpretentious buzz administered by means of a head-phone.

The same neglect as to the above has also been accorded to the closely allied fact that it is no matter of indifference whether and how the reactor can influence this change in the sensory field by his act. For it has been our experience that, as a rule, the reaction-time also turns out slightly differently when the reactor is or is not able to undo by his movement what has happened in the sensory field, to 'confirm' or to 'negative' it. In the first case it is considerably longer than in the second. Table II will give an idea of this.

TABLE II

RELATION BETWEEN NATURE AND TIME OF REACTION
(p = ca. 20% of t)

Field-occurrence	Reaction-time (self-time)
reaction (negatived)	t + p
reaction (confirmed)	t

The experiment came to this, that in one case the person experimented upon undoes by means of his reaction the change in the sensory field brought about by the signal, while in the other case he maintains the change by means of it. But it was conditional that he should know himself to be perfectly free regarding his tempo, so that, for instance, by occasionally not reacting immediately or by reacting slowly, he could ascertain that he himself did indeed control the disappearance or the remaining of the signal. For this reason we spoke of "self-time" in Table II as compared with "run-time" in Table I. For in the latter case one is emphatically adjusted to act as fast as possible; one is really in what we call a racing mood. The results obtained are so convincing that it will surely be believed that it was not without intention that we used light-spectacles and head-phone in this case too.

Of how great importance also is the significance of the signal's spatial mode of appearance became clear to us from the following experiment, which took place in connection with the traffic problem of the so-called 'fright-second.'[2] It concerned the measurement of the reaction-time when lighting a red lamp at various distances from the reactor.

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[1] Jacques van Essen, Bewegungskoordination bei monokularer Ganzfeldbeleuchtung, *Zsch. f. Sinnesphysiol.*, 66, 1936, 247-261.

[2] Van Essen, Reaction to distant and to near light-signals, *Ned. Tijdsch. Psychol.*, 4, 1936, 7-16.

Table III below shows the results obtained in the case of 14 adults, each of whom reacted at least 100 times at every distance. Of course, the influence of perspective on the size and intensity of the retinal image was taken into careful account.

TABLE III

REACTION BETWEEN DISTANCE AND REACTION-TIME

(p = 5–15% of t)

Distance	Reaction-time
2 m.	t + p
12 m.	t + p
24 m.	t

From this experiment it becomes clear that, as regards sight, the reaction-time becomes smaller as the distance increases. We have explained this by assuming that what happens in our immediate vicinity naturally demands a more thorough sensorimotor adjustment than what takes place at a distance, and so costs extra time. This is also experienced subjectively in the more or less paralyzing effect of sudden lighting close by as compared with in the distance. What is near has more *Leibhaftigkeit*, as it has been expressed, and it thrusts itself more emphatically upon the observer. But also from a biological point of view do occurrences in one's immediate vicinity naturally demand entire motor readiness to a much more imperative degree as compared with those which only impress us from afar.

With this we will close our discussion of the influence of objective circumstances, although, as every one knows, this subject is not by any means exhausted by the above. But it is now time to occupy ourselves with the corresponding influence of internal, or subjective, circumstances. For, in psychological time-measurement, attention was no less turned in that direction from the very outset, because it does really seem that they strongly influence the phenomenon of reaction. Thus we think of the influence of effort of will and attentiveness, or of change of disposition through practice and use, tiredness and restoration, boredom and interest, spontaneous automatizing and unintentional rhythmizing. They are all reasons why of the reaction-time in successive measures of the same individual hardly ever turn out to be the same, even though great care is taken to keep the objective circumstances the same. The more delicate the range of measurement, the clearer this is; and the more one thinks about it, the more one sees how inevitable it is. For the nature of effort of will and attentiveness is periodic, whereas the above-mentioned changes of disposition lie deeply moored in the natural functioning of our psychophysical being.

All these factors so strongly mark a single reaction-time with the stamp of accident that for a long time a large number of experiments have always been made in succession, and then the average was taken, which was then further characterized with some index of deviation. Viewed superficially, it does indeed look as if one has mathematically a right to do so. For no one can escape the impression that the time-values obtained in serial measurement normally group themselves around a central value. On this ground, one generally does not even shrink from applying the same mathematics as is customary in biometry; namely, the computation of chance supplemented by the theory of the probable error. So one starts from the point that, in serial meas-

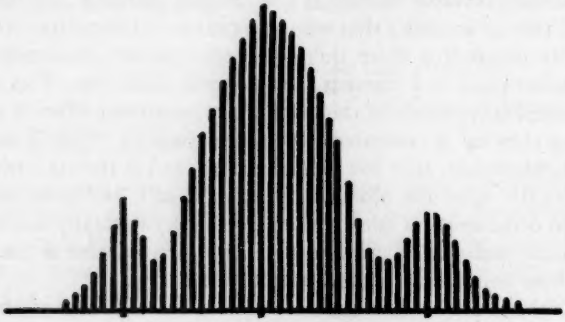

FIG. 1. FREQUENCY-DISTRIBUTION OF REACTION-TIMES

urement of reaction every separate time-value occurs, in principle, less frequently in proportion as it deviates more from the most usual; and vice versa. In the ideal case, the frequency-graph deducted from this resembles the well-known bell-shaped figure of Gauss as constructed from Newton's series of coefficients of the binomial theorem.

Now according to our views all this is not quite correct, which can be proved by facts as well as by argument. Thus, to begin with, it is not *practically* true that an ideal reaction-graph follows the accidental curve of Gauss. For, according to the results before us of a very large number of measurements, a normal and complete reaction-graph has at least *three* peaks.[2] There is, namely, a central principal peak, flanked on the left and on the right by a smaller secondary peak. (Fig. 1).

Concerning these peaks it is perhaps possible to think of the so-

[2] Van Essen, Concerning the construction of a normal reaction-graph, *Ned. Tijdsch. Psychol.*, 5, 1937, 34-49.

called muscular and sensory types of reaction and to look upon the central principal peak as the 'natural' reaction of Wundt, consisting of a mixture of the other two. But this certainly does not account for all cases. In our opinion they are much more an expression of the fact that, in the serial measurement of reaction-time, the reactor *fluctuates between at least three different modes of sensorimotor sets*. The one occurring oftenest leads to optimal time-achievement, and the two others to maximum and minimum time-achievement. By persevering measurement, this became clearer and clearer to us. Anyway, it is incorrect simply to look upon the running to extreme values in a reaction-graph as answering in principle to the binomial formula. If one really wishes to apply that formula, it is well to realize the reaction-graph as being at least *threefold binomial*, which, however, will hardly yield psychological advantage.

Meanwhile, we must not accept *in theory* either that the reaction-graph answers to Gauss. For this purpose it would be well to compare it with the most classical example of the biometry of Gauss; namely, Quetelet's measurement of the physical height of a levy of recruits. In this case, the fact that one recruit is a little taller or a little shorter than the preceding one is indeed a biological accident, because the individuals measured are, as a rule, genetically far removed from each other, and the entire phenomenon is further dominated by the purely individual functioning, in greater or less degree, of the factors of physical growth. The mathematical theory of probability may, therefore, be applied here, though here again we may not forget that the different variations are not purely mathematical or sheer chance, but actual biological reality.

In these reaction-times, however, we have to deal with *a perfect hierarchy of psychophysical performance*. For in this case each response is determined by what preceded it as well as by what will follow, because it forms part of the same systematic series of actions of the same individual. That is to say, when, in serial measurement, one response turns out temporarily somewhat different from another, we are not primarily concerned with an accidental quantitative fluctuation in performance, but rather with *a qualitatively different way of responding* founded on a *qualitiatively different mode of sensorimotor process*. The fact of surrounding a central value is here in reality but an expression of the general inclination towards consistency of psychophysical dynamics in the case of similar external circumstances and as such it has nothing to do with accidental variation. Indeed, in psychophysics one must al-

ways be careful not to dull one's vision by staring so hard at the traditional mathematical interpretation of the phenomena that one becomes apt to underrate and overlook their vital reality.

Allow me to explain this further by an example that at the moment lies very near to my heart. This spring there is a bird in my garden that every morning sings his rather monotonous and endlessly repeated song of "toet . . . toet . . . toet . . .," generally a repeated series of four times "toet" in succession, but not infrequently fewer or more than

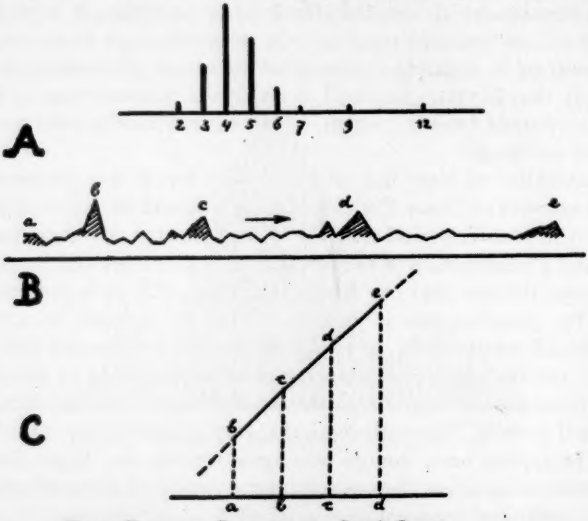

FIG. 2. FREQUENCY DIAGRAMS OF A BIRD'S CHIRP
A = Frequency-distribution; B= Frequency shown in order of occurrence; C = Plot of strength of impulse.

four, and even as many as twelve times. One morning I took all this down, and drew the frequency graph of it (Fig. 2A). It makes an impression of being very much like a Gaussian curve and one might think that the series around the central series of four times "toet" are nothing but a more or less accidental increase or decrease of that number, possibly because the impulse of utterance shows inevitable fluctuations. However, the reality is much more interesting.

In Fig 2B, the "toet"-series, as sung by the bird, are arranged in chronological order. What strikes us in the first place is that the longest series form themselves more or less close together but gradually grow farther apart. If this is again expressed graphically, we get the

straight line of Fig 2C. From this figure it is clear that the bird repeatedly gets a strong impulse of utterance, but that this impulse gradually fades out, and the process of fading out is mathematically a purely linear function. Both the beginning and the end of the song are characterized by one of these strong impulses, that is to say, as a real spontaneous performance there is also a real start and a real final spurt. We could not have read this from the frequency-graph, no matter what mathematical manipulations we had applied to it. Though in form exactly the same as any biometric frequency graph, it is therefore, like the reaction-graph, an expression of an *individual historical event*, com-

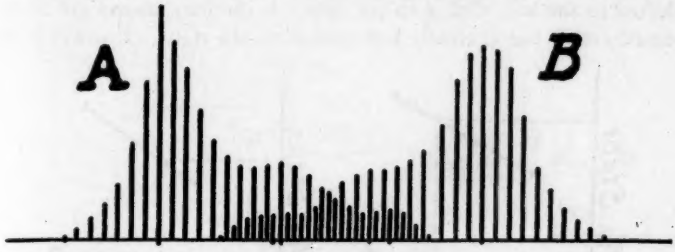

FIG. 3. FREQUENCY-DISTRIBUTION OF REACTION-TIMES MADE UNDER THE DISPOSITION FOR SLOW AND FOR RAPID
(A = Reactions made under the disposition for slow; B = reactions made under the disposition for rapid.)

pared with the biometric frequency graph, which only shows the *actual condition of a group*.

The clearest confirmation of the above in its mutual relation is the fact that a reaction-graph can strongly vary individually without giving the impression of being more normal in one case than in another, or of having been more influenced by disturbing elements. In other words, taken in a sufficiently wide sense, the study of the reaction-graph should also be of use for typological classification or characterological determination of the reactor. As far as this is concerned, we regret to say that as yet we have no distinct data at our disposal. But we have succeeded in finding out from the form of the reaction-graph something about the attitude of the reactor towards his task of observation and motion.[4]

More salient in this respect was the great difference between being 'set' or disposed for 'slow' and 'rapid' reactions. One should realize

[4] Van Essen, The reaction-graph as characteristic of mode of response, *Ned. Tijdsch. Psychol.*, 5, 1937, 144-159.

that the position on the time-scale does not give any decisive answer to this whatever. For it only tells us something about the objective shortness or length of the average time made. Yet an objectively short time may be very slow for the reactor, or an objectively long time may be very rapid. For this is exclusively determined by the relation between his temporal capacity and his exertion or subjective quickness.

Now, in our experience, the reaction-graph in the case of slow and rapid looks like Fig. 3.

In the first case, A, the high point of the distribution has been clearly shifted to the left, that is to say, towards the long times; and in the second case it has as clearly been shifted to the right, *i.e.* towards the

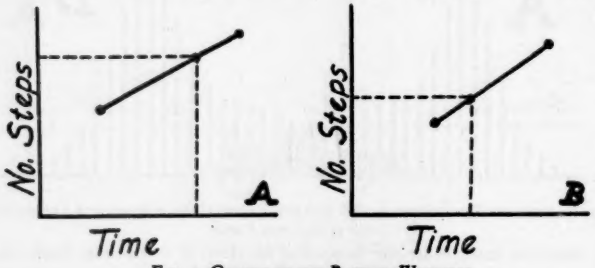

FIG. 4. GRAPHS OF THE RATE OF WALKING

short times. It seems as if in one case the right, in the other case the left secondary peak of the normal graph has become the principal peak, at the expense of the central peak. These facts seem to us another argument in favor of the opinion that a normal reaction-graph shows three peaks because of a fundamental triplicity of sensorimotor set, as fully stated above.

But let us now tell how we were able to ascertain the above. The first occasion was afforded by the behavior of the reactor. A person who does his utmost to be quick behaves differently from one who does not. But this is, of course, by no means an absolutely objective norm, so we first tried a further verification with the records of introspection. Unfortunately, this was too often unsuccessful, for deliberate self-observation disturbs the mental state of the reactor to such an extent that the experimental picture is almost entirely confused by it. For this reason, should we not also be very careful when we hear of the results of many classical experiments with regard to the so-called muscular and sensory responses?

We then proceeded to link the graphs under discussion with a determination of the reactor as 'slow' or 'rapid' for quite a different reason. This concerned an investigation of the subjective norms for the motor tempo when walking. The series of experiments of importance here was as follows: The task of the subject was to cover a distance of about 60 m. at different tempos in varying order; namely, 'slow,' 'ordinary,' and 'quick.' In the determination of those tempos the subject was entirely free to do as he liked and, when once he began to walk, he hardly had time to think about it, for at the same time he

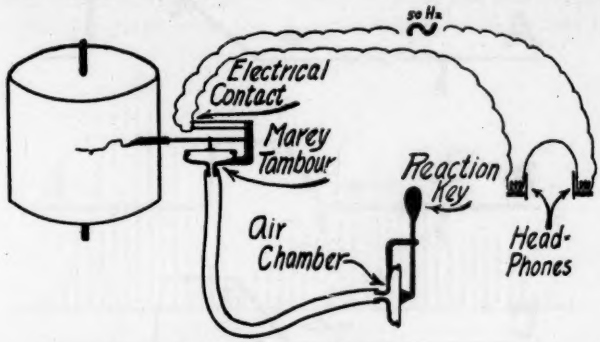

FIG. 5. DIAGRAM OF APPARATUS

had to count his steps. We then observed that there are people whose 'ordinary' tempo in walking is strikingly near 'slow' and others whose 'ordinary' walking is almost identical with 'rapid.' All this is graphically represented in Fig 4 A and 4 B.

On the base line the time is marked, and on the ordinate the number of steps; and that in such a way that the number of steps made at 'ordinary' is exactly as far from the zero of the scale as the number of time-units used with the same tempo. In our experience, 'ordinary' then generally comes just on the line connecting 'rapid' and 'slow,' so that in the case of every subject the connection between the size of the steps and the rapidity of movement can be determined as a single linear function. For instance, in Fig. 4 A, the walking-graph is that of a person whom, as far as his locomotion is concerned, we may with a clear conscience call 'slow.' Fig. 4 B is of a subject whom, with equal justice, we may describe as 'rapid.'

Now we are specially interested in that differentiation here. For *in various cases* we have been able to ascertain, in serial measurements of

visual and auditory reaction-time, that the former person produces *by preference* a reaction-graph as in Fig. 3 A, and the latter as in Fig. 3 B. We say purposely *in various cases* and *by preference*, for the connection was not always clear, though on the other hand it was always sufficiently striking to preclude the idea of accident. Anyway, the exceptions are very easy to explain. They are to be found in subjects who are not slow or rapid at all times and in all their performances, but are slow in one thing and rapid in another. Thus we had no absolute certainty

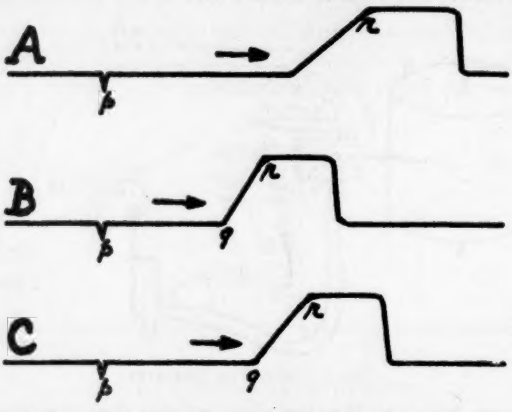

FIG. 6. TYPICAL CURVES OF THE REACTION-EXPERIMENTS

as yet that the graphs represented in Fig. 3 are indeed of persons who are disposed to be slow and rapid respectively. We had no certainty until we proceeded to analyze the response-movement into its parts. We did this on the ground of the reflection that if a person is indeed disposed to be rapid or slow, this should appear in the first place from the *tempo* of that movement.

The experiment in question was arranged as follows. The reaction-key was connected by means of a lever with an air-chamber, which was connected with a Marey tambour so that the slightest motion of the key was recorded on a kymograph. The signal was given by pressing an electric contact which was fitted upon the tambour so that at the moment of the closing of the circuit the writing-lever received a slight jerk, which distinctly marked the moment of stimulation (Fig. 5).

Fig. 6 affords an impression of the writing-lines thus obtained.

Stimulation took place at *p*, *i.e.* the reaction-signal was given. Then follows the creative interval in which the movement is prepared and which we call 'reaction-time,' after which, from *q* onwards, the course of the reaction-movement is registered.

It will be clear that if the distance *p-q* marks the length of the reaction-time, the rapidity of the movement may be seen from the line *q-r*. For the more this line slopes, the slower the movement will have been, and the reverse. So here we have indeed a sufficiently sure and sufficiently objective norm from which to conclude whether the reactor was disposed to be 'slow' or 'rapid.' Now the fact that the person with a reaction-graph as in Fig. 4 A produces a motor picture as in

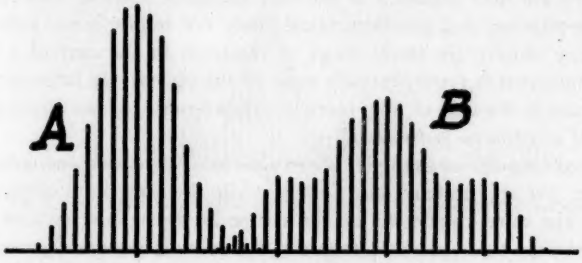

FIG. 7. REACTION-GRAPHS

Fig. 6 A, and the one in Fig. 4 B a motor picture as in Fig. 6 B, gives us a right to look upon the graph of Fig. 4 A simply as characteristic of the disposition 'slow' and that of Fig. 4 B as characteristic of the disposition 'rapid.'

We repeat that the length of the reaction-time has no immediate connection with this. In principle, it is all independant of the position of the average of the reactions on the time-scale. However, this latter does not mean that position on the time-scale is of no importance, quite apart from the possibility thus offered of making quantitative comparisons regarding the temporal capacity of different individuals. This is perhaps the right place to elaborate more fully upon this.

If we compare a reaction-graph consisting chiefly of shorter times with one that lies lower on the time-scale, we are struck by the fact that with an equal number of cases the former has by far the smaller average deviation, while the frequency columns are relatively high and the secondary peaks show to far less advantage (Fig. 7A and B).

The explanation of this will not be difficult if we consider the range

of variation as a scale for drawing conclusions. For the relatively small range of variation in the case of the higher position on the time-scale, together with the fact that the secondary peaks show to less advantage, means that a person with a naturally shorter 'run-time' is capable of combining rapidity of movement with concentration of attention. In the meantime this conclusion is not by any means justified, and in daily life, indeed, we not infrequently find a greater readiness for action, and also typical concentration of attention, in the case of slower natures. It would, therefore, seem more correct to seek an explanation of the combination of short 'run-time' and small range of variation, and of long 'run-time' and large range of variation, in something quite different. We are here thinking of the real difference there is between objective-physical and psychophysical time. For might it not rather be that the objectively small range of variation in the case of a short reaction-times is *psychophysically equal* to the objectively large range of variation in the case of long reaction-times *because both are identical as a norm of sensorimotor performance*?

Let us consider an example. When a perfectly equipped and conscious person A is able to react in a time t, while the same kind of person B under the same conditions makes a time $2t$, then it is obvious that, viewed from these persons, both times are equal, because they concern the same performance. They are not equal in an objective but in a psychophysical sense. It may then very well be that the objectively smaller range of variation of the one is psychophysically equal to the larger range of variation of the other, though all this has, of course, its limits.

But by reasoning thus we are doing the unusual thing we pointed out at the beginning of this treatise; namely, measuring time by reaction and not reaction by time. And we like to do so, for it enables us to penetrate much deeper into the problem of "time and reaction" than was possible hitherto and to form a much clearer conception of the construction of a normal reaction-graph. Thus a logical consequence of the above is that the customary display of the frequency columns on an arithmetical time-scale is, properly speaking, psychologically incorrect. It would be correct if every objectively equal time-space were also psychophysically equal, and that is not the case. For *the higher the position on the time-scale, the more performance units of reaction-times are included in the same objective time-unit, and vice versa*. Expressed mathematically, a reaction-graph should, therefore, be drawn on a geometrical and not on an arithmetical base line.

Let us consider another example. When in 'run-time' a person makes a reaction-time *t*, and in 'self-time,' so much lower on a time scale, a time *T*, then in the first case it will cost him infinitely more trouble to shorten his time by a few percentages than in the second. But a person with a naturally shorter reaction-time *t* also finds it much more difficult to make his time a little shorter than another who spontaneously makes the relatively much longer time *T*.

The more one thinks about the above, the more obvious it becomes. Phenomena which were hitherto inexplicable or were considered as unimportant now assume a deeper meaning. In this connection we are especially thinking of Wundt's so-called *Fehlreaktionen*, responses that

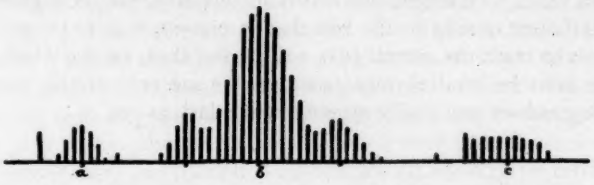

FIG. 8. FREQUENCY-DISTRIBUTIONS OF REACTION-TIMES SHOWING *Fehlreaktionen*

are too early or too late, which as such are really quite outside the occurrence in time to be measured. Normally they form a group by themselves on the left and on the right of the actual reaction-graph, and especially the late responses are not infrequently separated from them by an empty time space. And it is striking that as a rule the last-mentioned are *much farther* away from the central value and spread much more than the early responses (Fig. 8).

The explanation of this phenomenon is now quite obvious. The early responses congregate in a much smaller part of the time-scale which is psychophysically richer in performance than the late responses. The latter can even spread over an unlimited distance on the portion of the time-scale intended for them, though this is of course limited by the functionality of the sensorimotor datum.

The fictitious responses, hitherto always observed with a certain degree of annoyance and carefully kept outside the calculations, have thus suddenly become of importance. They become more so when we consider that their position on the arithmetical time-scale may be of valuable assistance in determining the geometrical time-scale on which the frequency columns have to be measured. For this we might perhaps take, in Fig. 8, the ratio of the distances *a-b* and *b-c*, in other words,

the tangent which is determined by the position occupied by the central time-value of the normal responses with regard to the early and late responses. So that, therefore, $R = (T_n - T_e)/(T_l - T_n)$, if R is the exponent of the geometrical progression of consecutive units of psychophysical time of performance, T_n is the central time-value in the case of normal response, and T_e and T_l those in the case of early and late response.

Apparently this method might also be applied by studying the proportions within the 'normal' part of a reaction-graph. For it stands to reason that, strictly speaking, the secondary peak of the very shortest times will be nearer the central main peak than that of the very longest times (Fig. 1). However, our efforts in this direction have generally been a failure, owing to the fact that in practice it is so exceedingly difficult to mark the central peak values. But then, on the whole, the above must be handled very carefully. We are only at the very beginning and are practically groping in the dark as yet.

THE PROBLEM OF SPACE WITH PARTICULAR EMPHASIS ON SPECIFIC SENSORY SPACES

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I. The General Problem

(*1*) *Universality of the problem.* The investigation of space offers a universal problem of great significance and of almost unlimited extension. If a general science of space should exist, it would have to include a group of problems which, for two centuries, have determined scientific research. Such a theory would have a more varied history than almost any other fundamental concept ever had. Psychologists, mathematicians, and natural scientists are all striving to solve this riddle, but, because of its two-fold origin, it is practically inexhaustible. On the one side it reaches into the field of reason, such as metaphysics, epistomology, and mathematics, while on the other, it appears in many different ways in our mental and physical world.

The universal character of space becomes apparent from the fact that frequently the same problems of space occur in the different theoretical sciences. In fact it already appears in one of the fundamental questions of philosophy.

(*a*) *Philosophy.* As it is known, philosophy raises the question of apriority and aposteriority of our perception of space, *i.e.* whether our concept of space is innate or developed from our experiences. This question, which has caused bitter controversy between nativists and empiricists of the previous century, and which had apparently been settled according to the idea of Kant, has not as yet been solved. This question is reappearing in a new form and is brought up from a philosophical and mathematical angle by Helmholtz,[1] Mach,[2] Poincaré,[3] Heymans,[4] Brouwer,[5] Geiger,[6] Schlick,[7] Weyl,[8] and Carnap.[9] The

[1] H. Helmholtz, Über den Ursprung und Sinn der geometrischen Sätze, *Wiss. Abh.*, 2.
[2] Ernst Mach, *Beiträge zur Analyse der Empfindungen*, 1885, 1–208.
[3] H. Poincaré, L'espace et la géométrie, *Rev. de mét. et de mor.*, 3, 1895, 631–646.
[4] Gerardus Heymans, *Die Gesetze und Elemente des wissenschaftlichen Denkens*, 1905, 1–421.
[5] L. E. J. Brouwer, *Intuitionalisme en formalisme*, 1912, 1 ff.
[6] Moritz Geiger, *Die philosophische Bedeutung der Relativitätstheorie*, 1921; *Systematische Axiomatik der euklidischen Geometrie*, 1924, 1–271.

problem of apriority of geometrical perception (cognition), its independence from experience, form a part of this series of questions (Kepler and Gauss as opposed by Helmholtz and Riemann).

If one interprets Kant's idea of the innateness of space in such a way that space as such is inherent, which means that the spatial character and order of things are inherent rather than the idea of the experience of space, then the contradiction between Kant's theory of space and that of the mathematicians disappear. No one would maintain that the spatiality of things is the result of our experience. In other words, one would not say that the character of our sense perception is such that the objects are placed externally to our body in such a way that we experience a certain specific order and a definite relationship of things to our self. This experience is no more direct than is that of color, tone, warmth, etc., or the recognition of relationships such as equality, similarity, and difference. This interpretation of Kant's idea offers the advantage that according to it the innateness of space is also valid for the animal world. This would at least be the case in those animals who have sense organs like or similar to ours. One need not assume that a highly organized animal, after birth, when its motor apparatus becomes mature, lives in chaos rather than in a world of order. This theory of innateness does not presuppose any precise type of space, not even tridimensionality. Consequently it can not be touched by the criticisms of those mathematicians and physicists who are against the assumption of an innate concept of tridimensional (Euclidean) space. Furthermore, the above interpretation allows a separation of the metaphysical problems of space from those dealing with the geometrical and physical side.

(*b*) *Metaphysics*. Philosophy has also to deal with space from the point of view of metaphysics. It has raised the question about the nature and the phenomenal reality of space, and investigated the character of imaginary and real space. It has also dealt with some other questions which cannot be understood through experience because they are prior to all experience: such as the questions concerning the concepts of eternity, unity, the singularity of space, continuity, divisibility and indivisibility. These are epistomological as well as metaphysical and mathematical problems. They explain the close bond between philosophy and mathematics. Furthermore, it was be-

[7] M. Schlick, *Allgemeine Erkenntnislehre*, 1925, 1–375.
[8] H. Weyl, Mathematische Analyse des Raumproblems, *Phil. d. Math.- u. Naturwiss.*, 1927, 90–100.
[9] R. Carnap, Der Raum, *Kantstud.*, Ergb. 56, 1922, 7–67.

cause of the splendid and rapid development of mathematical logic and mathematical epistomology, and because of the more recent investigations in geometry and physics, that the mathematicians and theoretical physicists took the lead with these questions, originally and fundamentally philosophical in their nature.

(*c*) *Geometry*. In terms of theoretical science the true science of space is geometry, *i.e.* mathematical and physical geometry. The first one attempts to build a complete and consistent and, if possible, a simple system of axioms, and from them to derive the geometrical theorems. At the time when we possessed only Euclidean geometry, which is the geometry of the homogeneous, static, and tridimensional space, *physics* had no other possibility than to demonstrate those experiences which give us space knowledge in terms of geometry (Natorp,[10] Geiger[11]). Since it was shown, however, that space need not necessarily be conceived in Euclidean terms, and that other types of space could be thought of which can also be proved mathematically, and which are as easily understood, and the laws of which have even more general application than those built on Euclidean geometry, a new problem arose for the physicist. This is the problem of choosing that type of theory of space which agrees the closest with the theories and discoveries in physics. That they decided against an Euclidean structure of space is generally known (Planck,[12] Einstein[13]).

(*d*) *Perceptibility of space*. The problem of the perceptibility of space must be separated definitely from the mathematical and physical problems just mentioned. It is a psychological problem, although physicists and mathematicians try especially hard to solve it. Helmholtz was the first to attempt to make non-Euclidian space perceptible. His demonstration of the perceptibility of spherical and pseudo-spherical space has been reduced to a two-dimensional structure which consequently is the Euclidean system of space. The same thing is true about the famous model of Klein.[14] These models merely give a perceptible demonstration (pictorial) of non-Euclidean relationship and configuration in Euclidean space. From the treatment of this specifically psychological

[10] P. Natorp, *Die logischen Grundlagen der exakten Wissenschaften*, 1910, 1–416.
[11] *Opp. citt.*
[12] M. Planck, *Das Weltbild der neuen Physik*, 1929, 1–52; *The Universe in the Light of Physics*, 1931, 1–110.
[13] Albert Einstein, *Über die spezielle und allgemeine Relativitätstheorie, und die Grundlage der allgemeinen Relativitätstheorie*, 1929, 1–64.
[14] Felix Klein, Über die sogenannte nicht-euklidische Geometrie, *Math. Ann.*, 37, 1890, 544–572.

problem there resulted great confusion, because in scientific terms the concept of perceptibility or palpability had been defined in various ways. If we would have strictly differentiated between sensory experience and ideational conception this problem of the perceptibility of non-Euclidean space would have caused no such confusion. Then one would not have insisted on conformity where such is fundamentally impossible because of the very heterogeneity of these two aspects of experience.

(*e*) *Art*. Entirely different problems of space are found in art. These problems grow out of art. Art does not reflect upon space, rather it creates and demonstrates it.

Architecture builds space. It creates artistic space of autonomous character, and this space owes its existence to free and creative ideas. The artistic task of architectural space-building has nothing to do with reality, with the reproduction of the corporeal, the cubic, the mass. On the contrary, by means of enclosure, bordering, and systematic arrangement of the structural mass, we attempt to create structures of esthetic nature; floating, non-material, but at the same time limited and restricted space-structures. This independent creation of space is found in its highest form in the interior rooms of Roman halls, in Byzantine domes and in the baroque structures of the interior of the churches of the middle ages.

The other major problem of creative art which is dealing with the demonstration of space is found in flat art, particularly painting. The task of art is to represent the spatial impressions of the eyes in an artistic manner. Here the problem consists not in reproducing nature correctly from the point of view of perspective, nor in reorganizing and reinterpreting tridimensional space and space-objects into a flat picture of exact likeness, but in making objects and spaces perceptible, and in reorganizing depth and distance so it will be esthetically and sensorially apprehended immediately. The fact that by so doing one is employing such principles of space as perspective, abbreviation, superposition, and the color effects which are changed through atmospheric conditions, is not contrary to the principles of freedom of artistic creation.

(*f*) *Biology*. Space does not attain complete subjectivity before we enter the realm of biology. Biological space does not correspond to the geometrical or the physical, and still less to the space-image which is represented by creative art. Biological space consists in that part of the external world where the actions and reactions of living organisms

take place, and it includes that field out of which the receptor organs receive their stimuli. Stimuli which come from beyond the boundary of biological space are unable to induce, whether in men or in animals, any positive or negative reactions.

The field of biological space is variable. It depends most upon the type of the living organism, upon the grade of development, upon its perceptive organs, and upon its psychophysical organization. Because of this, the biological space of an eagle is larger than that of a gull, the sphere of action of a bee is broader than that of a spider, and the sphere of perception and behavior of an adult is considerably more comprehensive than that of a small child, even though the latter has fully developed sense-organs. The limits of biologically effective space vary also according to the environmental configuration. For example, the appearance of a dangerous and fear-arousing animal into the hen yard increases the biological space of the hens, while quiet and undisturbed environment decreases it.

The sensory structure of the space configuration of animals can only be determined in an indirect way. This task belongs to comparative biology and animal psychology. They have to investigate the problem through experimentation and by observing organ development, the motor apparatus, and the spatial localization and orientation. Although experimental animal psychology has enlarged our knowledge along that line tremendously, the difficulties have not as yet been completely conquered, not even in the understanding of the most natural and instinctive movements. If what are regarded as the simplest movements of the lower organisms (the tropisms, the reflexive approaching and withdrawal movements of an animal organism in response to a stimulus) lead to contradictory hypothesis, how much more difficult will it be to explain more complex behavior, as for example that of the storks of Amsterdam who, without special guidance, migrate to Egypt and always return to Amsterdam at the correct time. Just by what external signs the different animals are guided in their migration is a question that will probably be answered in time, but in what ways they receive and employ these clues remain problems the final solution of which, because of theoretical reasons, will probably never be found. Despite the fact, however, that we may not be able to penetrate the nucleus of the sensory life of animals, even after acquiring the most precise knowledge of their sensory organization, our scientific spirit, which is striving and searching, may still bring us to those heights where the incomprehensible will become comprehensible. Logic and

intuition, experience, experiment and theory, will form the bridge to the beyond of immediate experience.[15] Thus, the general biological problem of space leads us directly to our specific topic—the psychological problem of space.

II. Sensory Space

(*1*) *Space perception.* The analysis of space perception is related to the visual, auditory, and kinesthetic functions. As a rule, under the title, "Perception of visual, tactual, and kinesthetic space," one discusses in psychology the facts and theories of this field. According to my opinion, this is a superficial classification, and is partly to blame for the fact that psychology of space frequently neglects some general points of view, with the further consequence that some problems of fundamental significance have not been raised at all, or only passingly touched upon.

I shall refrain from enumerating the many problems which occur in the field of space perception. I shall limit myself to a brief demonstration of a few fundamental problems, which have particularly occupied my mind during recent years, and which, as it seems to me, opens to us a lead toward a *general* theory of space, one which will include all perceptual material.

Let us start with the qualities of sensory space. My investigations have convinced me that there are different, independent, and qualitatively unrelated sensory space-structures. A complete analysis of them fails because they are so closely interwoven that a palpable separation of them is impossible.

In *visual* perception of the corporeal world, sight, touch, and movement are active. A pure visual space-image which is entirely free from all foreign elements is not possible because of the simple fact that communication from tactual-kinesthetic functions cannot be excluded. It would be far more probable to realize *haptic space* and haptic space-images because it is quite possible, at least for people who were born blind, to exclude visual images; but those who see can receive only a partial idea of pure tactual space and space-images perceived through

[15] Although, according to most reliable information, the experience of space and perception of objects of animals seem to agree with that of our own, the theory of a general phenomenal agreement between animal and human perception is highly disputable from a logical and theoretical angle. In a study, which soon will appear in the *Journal of Psychology*, I shall offer proof that, because of lack of language and ideas, all animals must have a different space concept, and that their objects must be perceived in a fundamentally different configuration and order than ours, and that must be the case regardless of their particular stage of evolutionary development and their biological relationship to man.

touch, because an act of touch automatically initiates also visual impressions or sensations. Even when this transformation or this visualiation of tactual sensations does not take place, we cannot obtain a pure tactual configuration. All our tactual sensations are penetrated and diffused with visual-tactual experiences, and our visual object perception is also not free from tactual experience. Even though the fusion of visual and tactual sensations takes place easily, we are still justified in postulating a tactual and kinesthetic space which is independent of visual space, that is to say, which can be described completely in terms of purely tactual sensations.

(*2*) *Visual and tactual space.* Visual, tactual, and kinesthetic space are completely developed sensory spaces. They are not, however, of equal importance because they do not give equal amount of knowledge about our space and corporeal world. From this point of view, visual space, as immediate and distant space, far surpasses tactual space, which appears only as an immediate space, and which even in that respect is very limited. Visual and tactual space are both tridimensional. That means that the visual and tactual space relationships can be analyzed into three dimensions—height, width, and depth. These concepts have to be derived from our perceptually given body positions.

A special kind of haptic space experience is caused by one's own body which one can experience when one moves about in free space with closed eyes.

(*3*) *Other sensory spaces.* Another question is whether or not we may attribute sensory space to other senses. This question applies specifically to our auditory sense, and then to all the other remaining senses.

(*a*) *Audition.* In the auditory field, the problem becomes quite difficult. Sounds, tones, and noises are all localized in visual space. It would never occur to the naïve observer to assume, besides a visual and tactual space, a phenomenally independent auditory space, where tones and noises find their place in the same manner as do the space objects in the visual-tactual space. Upon a close analysis it becomes apparent that the doctrines of a specific auditory space (Brentano,[16] Stumpf,[17] Pierce,[18] and Von Hornbostel[19]), are completely untenable.[20]

[16] Franz Brentano, *Psychologie vom empirischen Standpunkt*, 2, 1874, 262 f.
[17] Carl Stumpf, *Über den psychologischen Ursprung der Raumvorstellung*, 1873, 1–324.
[18] A. H. Pierce, *Studies in Auditory and Visual Space-Perception*, 1902, 1–362.
[19] Erich von Hornbostel, Das räumliche Hören, *Hdbh. d. norm. u. pathol. Physiol.*, 11, 1926, 701–730.
[20] In one of my previous publications I have treated auditory space as a rudimentary space. At present, however, I reject definitely any concept which grants an authentic acoustic space.

Neither the localization of objective tones in space, nor the subjective auditory images inside the head, nor the apparent space nature of our tone sensations (height, depth, and volume) justify us in assuming an auditory space which is equivalent to the visual and tactual space.[21]

(*b*) *The so-called lower senses.* Can we speak of a vibratory, olfactory, gustatory, and temperature space in the same sense as we talk of visual and tactual space? Not according to my opinion. These sensations, which are perceived either *in* or *on* the body and only very rarely outside of it, are always arranged in the visual and tactual space.

The sensations of the lower senses have in common with the auditory sensations that they cannot give us any knowledge of the structure of our surrounding space and of our corporeal world. Any being which would have to renounce completely all visual-tactual and kinesthetic sensations could probably discover (mainly through hearing) the existence of an external world, but it could not directly experience or conclude from logical inference as to *how* this world was organized, and *what* it contained. Such a being would not even be able to experience its own bodily self, presupposing of course that vibratory sensations may become separated from tactual sensations and be thus perceived alone. Neither would such a being become aware of the objectivity of spatial objects. The sensations from the lower senses give us no space knowledge because of the following reasons. In contrast to visual tactual and auditory stimuli, which are localized outside the body, we localize vibratory as well as olfactory and temperature stimulations as originating in our own body, and in the perceptual image of the stimuli, or in the immediate environment of the perceiving organs. All this presupposes visual-tactual experiences. We are able to immediately specify a stimulated spot on the skin without first referring to the stimulating cause, while we remain completely in the dark about the place of the stimulating source. Whether a cutaneous spot is warmed by means of electric heat or natural sun light cannot be determined from the nature of the sensation. Any being which is without the higher sensory modalities will have no criteria for judging whether a sensation was caused by external stimuli or by internal irritation. Its impressions will take on the character of emotions, and thus become a part of its ego. They will, however, seem to have *one* type of space specificity; namely, the *localization of direction.*

[21] I have dealt with the whole problem of auditory space, its history, a critique of the theories and solutions of the problems in, Gibt es einen Hörraum? *Acta Psychol.*, 3, 1937, 137–192.

There is no question but that one can definitely localize the direction of some specific olfactory and thermal stimuli. It seems to me problematic, however, whether one has here to deal with an immediate direction-awareness essentially belonging to the sensation, or whether the judgment is based on inference from the experience of other senses. If we hold strictly to the idea of specific sensory content, at which we arrive through analyzing and abstracting the experiential whole, it should become clear that we recognize the direction of stimulation only *indirectly* and from inference from the sensations which are localized on one's own body. From that experience we conclude that the source of stimulation lies either opposite, in front, or on the side of the affected skin area. More specific information about the direction may be obtained only when we carry out *movements*. With the aid of these movements one would try to find a direction from which the stimuli reach one with the greatest intensity, and this may be accomplished by means of the olfactory organ, or by means of vibratory or temperature sensations. The stimuli are then localized in this enlarged sensory field.

It can also be shown that in the field of the lower senses the existence of external stimulation, as well as its direction and its particular place, can be ascertained only by inference from past experience. In this process kinesthetic sensations play an important rôle.

Since the lower senses miss that type of function of space which we find among the higher senses, the resulting sensations, as of warmth, of vibrations, and of odor, if they become objectified and their stimuli located out in space, must be connected with an already existing ideational space, and this can be no other than the visual-tactual space.

This conception may easily be defended because localization of direction need not presuppose experience of space. According to my opinion, localization of direction is based on reactions which belong to the category of tropisms. The approaching of lower organisms toward light or warmth is principally nothing else than our localization of direction concerning warmth, odor, and vibrations. This recognition leads us to a conclusion which is very important to genetic psychology; namely, that those animals which have no higher senses and which must rely on their lower senses alone for their sense experiences can have no experience of space, not even in a rudimentary form. They are able to carry out direction-determining movements, but without having the slightest idea of the surrounding space, or of any order in their environment. We may state that the facts and observations in the acoustic field, and that of the other so-called lower senses, indicate

that we can not expect them to have any sense space of their own. Everything points to the fact that all our sensations, such as tone, vibration, odor, taste, and temperature, appear in the visual-tactual space. There exist only two authentic sensory spaces of which our complete objective world and our ideas of space are built. These are the spaces of the visual and the haptic senses. Other than those two there are none.

III. Optic and Haptic

Now let us turn to our third question. What structure and appearance have the fully developed sensory spaces? Since they all seem to have reference to one and the same space, are they essentially alike or do they differ from each other? We take the latter view, and we wish therefore to demonstrate briefly the *heterogeneity of the space and form world* of vision and touch. The difference between these space structures may be characterized best by pointing out the geometrical relationships between vision and touch, and by a comparison of the phenomenal differences of the configurational processes involved in the two types of form structures.

(*1*) *Relation of visual space to haptic and geometric.* The concept that there is only one type of sensory space, and that it is expressed in visual terms has been definitely contradicted. Not even the much more plausible idea that Euclidean space is best demonstrated by visual space, or, in other words, that visual space is that type of space by which we can demonstrate geometric theorems most effectively, can be defended any longer (Poincaré,[22] Mannoury,[23] Weber and Dallenbach[24]).

Another question which arises is whether or not it could be the haptic space that agrees in nature with the Euclidean. In my opinion Euclidean space does not correspond in reality with any subjective space. I believe, however, that it is probably more closely related to tactual than to visual space. This is contrary to the generally accepted view. Let me, therefore point out some reasons that justify it.

(*a*) *Structural differences.* First let us consider the structural differences. Visual or optic space has perspective, while the tactual and geometric spaces are devoid of it. This fact explains why in visual space the physical size of an object changes its size in proportion to its

[22] H. Poincaré, *op. cit.*; also Sur les principes de la géometrie, *Rev. d. met. et de mor.*, 8, 1900, 169–188.

[23] G. Mannoury, *Methodologisches und Philosophisches zur Elementam-Mathematik*, 1909, 1–276.

[24] C. O. Weber and K. M. Dallenbach, The properties of space in kinaesthetic fields of force, this Journal, 41, 1929, 95–105.

distance from the subject, while in haptic or tactual and geometric space the size of any given mass remains constant regardless of any changes in distance. Furthermore, visual space is limited, while haptic space, because of this freedom from the effects of distance, as well as geometric space, because of theoretical reasons, are unlimited. Therefore we find that in haptic space there is a compulsion to go beyond the limits of distinctness, and this force has its source in the phenomenal. Tactual, or haptic space is originally tridimensional, and, therefore, it alone may be expected to be more closely related to geometry.

Not the eye but the hand recognizes the physical character of things and their stereometric structure. Another condition which points to a close relationship between geometry and tactual space is the fact that in visual space one *estimates* spatial size, for which individual experience is of greater importance, while in geometry and the haptic space the opposite takes place; there, distance and size are *measured*. In geometry this is done with the objective and invariable meter, and in haptic space with the individual and invariable measure of finger and width of hand, and length of foot, etc. Haptic space may, therefore, be characterized also as a "geometric-space-measure," while one has to conceive of visual space, at least in part, as a projection space. It is by no means a coincidence that previous to the introduction of the metric system, measuring was done in terms of haptic units of measurements, such as the thumb, finger, hand, or foot. The metric nature of the haptic is particularly apparent from the fact that some of our measuring instruments used tactual units as examples for their units of measurements, of which the thumb and the index finger may be mentioned (Friedmann[25]). Another characteristic of the haptic is that the tactual object, as compared with the visual, is approximately constant; it is not so much subject to the changes of the phenomenal world. Such important factors as the play of colors, changes in atmosphere, the place from which an observation is made, changes in place of the object, all influence the nature of appearance of the visual object, while in geometry and haptic space they have no significance. In addition, the somatic and purely subjective factors are reduced to a minimum. Furthermore, in haptic space it is impossible to find analogies for anomalies in form and color vision. Only the illusions of the tactual field correspond to the illusions found in the visual sphere. These, however, are not anomalies. They should be considered, in a general way, as expressions of developmental factors of space-perceptive sensory

[25] H. Friedmann, *Die Welt der Formen*, 1930, 1–519.

processes (Révész[26]). Neither do our different moods or our general physiological condition, which, as is well known, have variable influences on our attitude toward nature, art, actuality and semblance, have any analogous influences upon the tactual and geometric spheres. We may, therefore, briefly state that through our tactual sense we comprehend *the constant, the approximate, the invariable, the general*, or in other words, *the sensory and perceptible content of geometry*.

That the perceptibility of geometric relationships does find its realization in the visual and not in the tactual does not contradict the above mentioned theory. This happens because of the peculiar development of our visual sense. By means of vision we are able simultaneously to survey and grasp large configurations of many single patterns, and thus, to make space relationships tangible (conceivable) in a general way.

(*b*) *Differences in content*. Besides these structural differences between visual and tactual perception there are still differences of *content and genetic characteristics*. In my investigations, which I recently reported,[27] the empirical evidence is given for my statements. This evidence leaves no doubt that the tactual configurations differ fundamentally from the visual with respect to their sensory content and perceptibility. The visual perception of an object may differ phenomenally so completely from the tactual perception of the same object, that it is not identifiable as the same object. It is not an exception to find that, if we have only touched an object, we do not recognize it visually. The same holds true the other way. Tactually distinctly differentiated geometric figures are confused when visually presented. Also the process of recognition takes place differently in the tactual and the visual field. Visual objects are immediately perceived as total and complete configurations, while tactual perceptions take place successively, and by constructing a total pattern from separate independent parts. Tactual form perception is fragmentary and furnishes only a schematic picture of the touched objects. Very seldom does it happen that a constructive formation of a perceptible *total configuration* takes place. That under these conditions tactual perception takes much longer than visual perception is obvious. In terms of perception time, it takes ten seconds to visually perceive a not too difficult asymmetric linear figure, while it takes

[26] Géza Révész, *Über taktile Agnosie*, 1927, 1–138; Über die Natur der optischen und haptischen Formwahrnehmung, *Proc. Konin. Akad. Wetensch. Amsterdam*, 34, 1931, (no. 1), 725–731. System der optischen und haptischen Raumtäuschungen, *Zsch. f. Psychol.*, 131, 1934, 296–375.

[27] Révész, *Die Formenwelt des Tastsinnes*, 1927, 1 ff.

fifteen minutes to perceive the same figure by means of touch. Not even after a long period of practice are we able to perceive tactually as quickly as visually. Even if we think that we have completely mastered the form of a given object by means of tactual manipulation, we are very much surprised at the change in the appearance if afterward we suddenly see the object. This change of impression depends on that characteristic which permits the visual process to perceive total configurations and which is absent in the tactual sense. Individual characteristics of an object cannot be brought out tactually because of the limited possibility for tactual perception of proportion of space (Blomhert[28]). In the visual field the proportions are indivisible marks of a configuration, such as the intervals in a melody. In the tactual field they do not belong to the constitutive, but to the constructive part of the tactual image. First they have to be perceived in terms of *measurements* and *estimates* before they can become a part in the constructive development of the perceptual image.[29] In the tactual field, the direct perception of objects and the perception of parts which are the preconditions of any esthetic perceptions are very limited. The tactual field refers only to the most simple and best known perceptual objects. Furthermore, tactual perceptions reach only the external, the surface of plastic works, and not the internal forces, such as the individuality and physiognomy of objects. All these factors lead to the conclusion that the tactually perceiving person, in other words the *blind*, must be almost devoid of all *esthetic perception of form*. We are not exaggerating when we maintain that the blind are outside the sphere of creative art, although it may be quite possible that plastic works may create agreeable sensory feelings in the blind. These experiences, however, are non-esthetic in nature; they arise not from the formal and beautiful, but from perception of the touched surface, and from the knowledge of the manipulated object.

This view seems to be contradictory to the theory of art by Riegl.[30] As may be known, Riegl differentiated three phases in the development of graphic art of the Middle Ages. First, a haptic phase, which

[28] G. Blomhert, Contribution to the study of the haptic perception of proportions, *Proc. Konin. Akad. Wetensch. Amsterdam*, 38, 1935, 931–942.

[29] I shall here not enter into the problem of the agreement between the achievement of the visual and tactual senses. The fact that both, and only those two, have the function of space perception and of configuration points clearly to a close relationship (Blumenfeld). Further indications of relationship are that visual and tactual sensations are subject to the same illusions. All the visual illusions occur also in the tactual field. Furthermore, both sensory fields have equal part in the structure formation of the phenomenal space.

[30] A. Riegl, *Spätrömische Kunstindustrie*, 1927, 1–421.

found its purest expression in old Egyptian art; second, a haptic-visual phase which, according to him, degenerated after the classic period of Greek art; and third the visual phase which is characterized by the later period of Roman art. From these phases many students of Riegl received the impression as if he stood for some specific principles of a haptic art, an art which would be built primarily on tactual sensations and experiences. Such an art does not exist, however, nor has Riegl ever postulated one. What Riegl attempted to accomplish was to show that the whole development of art is governed by contradictory principles, and in so doing Reigl called the one principle the haptic and the other the optic. From his point of view, these two concepts, "haptic" and "optic," mean nothing more or less than that there are two different attitudes in visual perception. If a person is tactually set toward an object there appear before him the kind of visual characteristics which deal with the material, corporeal and structural, and the detailed expression of the object. If one chooses a visual set or attitude, then the haptic or material characteristics recede into the background. In the first instance the visual organ anticipates the function of the tactual organs—it 'touches' the objects with the eyes, while in the second instance, the normal simultaneous synthetic perceptual character of vision comes into function. In each of these cases we deal with *vision*, although these are two different kinds of artistic visualization, called the tactual and the optic. The misunderstood term "haptic" in Riegl's theory of art connotes merely a way of visualizing or seeing which resembles the functions of the tactual senses.

Blind sculptors. The most heavily weighing argument against our theory of the limitation of the function of perception of space in the tactual sense, and the contention that in the blind esthetic experience is absent lies in the fact that there are blind sculptors, people who, in spite of their blindness, are able to create works of art. If this were true, then our point of view, that the haptic sense has no relation to esthetic experience of form, and that consequently the blind will be excluded from any esthetic enjoyment of plastic works of art, would not be correct and could no longer be held. What in reality is the fact about blind sculptors and their works?

My investigations of a great number of blind who mould and of eight blind sculptors indicate that those who were born blind, those to whom the visual world had always been closed, and who never perceived any objects or works of art through their visual sense, and whose total development took place without any visual experience,

are unable to give to their plastic works any definite and distinct form. Although their works do not lack expressive character, nevertheless, they do lack harmonic proportions. To express and create graceful forms is beyond their ability. This causes the expressionistic tendency in the creations of artists born blind. They try to express their own bodily feelings, and to fill their works with their own emotions and affections. The fact that their creations are limited to the feelings of their own body, is what lends life and expression to their plastic works. Sculptors who had received their training before becoming blind produce, during their early period of blindness, works which are in form very similar to those which they created before growing blind. During the progress of their blindness, however, they gradually lose their artistic individuality, and more and more their creations take on a schematic form, until finally because their visual memory images disappear, and the remaining ones gradually lose their connection with their tactual experience, their works lose their artistic content. In spite of my efforts, I have found only one blind sculptor (Masuelli of Rome) in whom the visual has apparently still such a strong influence on the phantasy and the formations of his hands that he still imagines and perceives in visual-tactual terms. In his case, this must occur in a similar way as in seeing-people who are permitting their past experiences to influence their tactual perceptions. This sculptor, by nature an artist, had the ability in spite of his blindness to preserve the visual sphere of imagery, which is the source of all artistic form, and because of this, his artistic activities are still controlled by the esthetic forms of the seeing.[31]

The last group of blind sculptors, those who only began to work with sculpture after their becoming blind, do not create anything worthwhile (Moundry of Prag, Scapini of Paris). I know only of one person not born blind, who supposedly became blind when four years old, who seems to have distinguished himself by strong artistic creative ability (Kleinhaus of Innsbruck). His case, however, remains problematic, in spite of all possible pains that have been taken to reconstruct his biography, and to analyze critically the works which have been attributed to him.

[31] The plastic works of these artists and of other blind sculptors are described for the first time in the second volume of my work "The World of Form of the Tactual Sense." Whether or not Masuelli will succeed in keeping up the continuity of his visual and blind world, and to continue in keeping those form principles which originated in his visual world, I consider an open question.

I have attempted in this study to demonstrate briefly the problem of space in its fundamental aspects and to show its relation to different branches of knowledge. My main objective was to raise the question of the autonomous sense-spaces, and to show how the psychology of space might be freed of some untenable views which cloud the psychological literature because of the confusion of psychological and philosophical approaches. At the same time, this analysis offered me the opportunity to draw the attention of my American Colleagues to some investigations which I and some of my collaborators have made during the past few years. I am hoping to arouse interest in some of these pertinent problems in the tactual-sensory field, a field which offers ample opportunity for research and which contains still a great many vital questions of major importance for general and for comparative psychology.

A COMPARATIVE APPROACH TOWARD THE PSYCHOLOGY OF COGNITION

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(Founded in 1936)

When I first took up the study of psychology in America, the Wundtian prejudice against making the psychology of cognition a description of human knowing was quite strong in certain quarters. The section on "Thought" in Titchener's *Textbook of Psychology* was probably meant to take the place of Wundtian "creative synthesis." As *pure* psychology, it was as far removed from a theory of knowledge as any existentialist or Buddhist could desire. Outside of Wundt's sphere of influence, 'scholastic' psychologists and practical educationists kept up a verbal fight on how we think.

A great deal of sound experimental work on the thinking processes has, however, been done within the last twenty years. The purpose is no longer to analyze or precipitate out 'meanings' and 'relations,' nor primarily to measure 'intelligence,' but to describe those processes which are conventionally called perceiving and judging. The present-day use of those terms is not limited to what is introspective but is extended to any human or animal response serving the same practical purpose.

Quite independently of theories such as 'isomorphism,' 'identity,' etc., the descriptive study of the thinking processes has enlarged its scope from the pioneering work of Binet and Ebbinghaus to cover "intelligence tests for apes" and more recently the researches of Lashley and his students on rats' perception and concept formation. It can even be said that the recent comparative psychology of the higher mental processes is justifiable and understandable more on a logical than neurological background. Right at this moment we may already be laying the foundations for a real 'experimental logic' which does not aim at teaching a rat how to think but tries to educate ourselves as to how it does think.

Shapes, Meanings, Relations

Wundt's disciples, Külpe as well as Titchener, left us high and dry as we looked beyond or looked back from the very special psychology of sensations. The Würzburg school re-introduced the meanings and relations which Titchener had no use for. But the experimental psychology of the thought processes is not limited to superadded meanings and relations. The problems of perceiving are inherent in the problems of thinking. In spite of the fusions and colligations, existential psychology really tells us nothing about perception except the uncomfortable promise that it can be reduced to sensations without remainder.

We are apt to think that *Gestaltpsychologie* comes in here to fill up a gap. This is probably because the Gestalt psychologists first introduced themselves to us through a doctrine of perception. While no psychologist ever held very closely to "bundles" and "constancies," we now know that these hypotheses cannot work. Configurationally speaking, the functional analysis of perception amounts to setting up fields of force for the animal to react to in a way that demonstrates its discriminative capacity. Being genetically as well as logically minded, one might wish to know more about perception in simple uniform fields, *i.e.* with equilibrium at lower levels. The work of Metzger on primordial space is most interesting.[1] The human nervous system does not usually react at such low levels.

The study of perceptual reactions which ought to be a concrete descriptive problem still hangs in the air. Space and time came in especially for trouble. Being neither data nor faculty, the philosophers did not know what to do with them. Mach, the predecessor of existential psychology, for instance, spoke of "sensations, times, spaces." A difference was made between *Sinnesempfindungen* and *Organempfindungen* on the one hand and between the former and the *Orientierungsempfindungen* on the other,[2] which only raised the problem. In psychology, the *Gestaltqualitäten* occupied the same place with perception as did the meanings and relations with the thought processes. It sounds as if the non-Wundtian psychologists have tried to tell us, for example, that this triangle in front of us is irreducible so far as its triangularity and its relation to other triangularities and non-triangularities are concerned. On the other hand, the extensities and protensities of

[1] W. Metzger, Optische Untersuchungen am Ganzfeld, *Psychol. Forsch.*, 13, 1930, 6-29.
[2] E. Mach, *Erkenntnis und Irrtum: Skizzen zur Psychologie der Forschung*, 5th ed., 1926, 345 ff., 434 ff.

Titchener seem to be nothing but imaginary projections of space and time as actually experienced. The doctrine of local signature and analogically that of temporal signature have served just one purpose, that of closing the door of psychology to the nativists and the positivists, but within the realm of psychology we have taken over all their time-worn argumentations.

Half a century of strict introspective analysis of perceiving and thinking has left us with a few irreducibles. One may be convinced that the comparative method is a more adequate method for tackling these problems and we must be outspokenly more systematic and philosophical in leaning.

The Objectification of Meaning

Of the three problems, those of meaning, relating, and perceiving, the first no longer occupies a prominent place in introspective psychology. It has resigned from introspective psychology. At the same time Titchener made his "contexture" unconscious, the physiologists took up what was ruled out of introspection. Meaning was associating for British psychology; it is now conditioning for the Russians and the behaviorists. The logicians have also taken up the same line of psychologizing. Of all the meanings of meaning, Ogden and Richards selected conditioning for elaboration.[3] The diagram on page 11 of their book serves the same purpose for philosophy as the salivary fistula for the dog. Beyond that, meaning as implication or as God's will simply *is* and nothing more can be said about it. This is at least true with psychology.

The Comparative Psychology of Perception

Coming back to perceptions and relations, one is at once struck with the fact that we have been belaboring with verbal difficulties, *i.e.* difficulties inherent with human language in general and the Indo-European languages in particular. The verbal report is human, all too human; the syllogism and the experiments on syllogistic thinking are European. Our attitude has been that the person either perceives as he does or else like a lower animal there is for him a big buzzing blooming confusion. So with relational thinking, one either speaks Hindu or Greek or does not think at all. The comparative method will give us a more proper perspective.

The simplest human perceptive response under controlled condi-

[3] C. K. Ogden and J. A. Richards, *The Meaning of Meaning*, 1930, Chap. 1.

tions is very definite and precise. When reported, the laboratory atmosphere and special philosophical instructions set up certain schemes, many taboos. Even without such artificialities human language is already too precise a tool for the description of perceiving. From the laboratory viewpoint the perceptions of daily experience are considered as compounds or emergences, as if there were back of human knowing a substratum of actual rudimentary things. I have already mentioned Metzger's work on primitive space-perception as representing a different line of approach. Now *Gestaltung* is also a set-up. Functional analysis will reveal many 'forces,' some arising from the environmental field and some supplied by the reacting animal. With human subjects, the intraorganismic forces can be controlled only to a very limited extent, especially the potentialities of the nervous system which the Gestalt psychologists emphasize most. The minute control of environmental forces usually becomes unimportant when the internal conditions are simplified or held constant. One of the methods of comparative psychology is to make the nervous system a variable and the most important one.

Let us take space perception as an example. We are far from being able to tell the story of the emergence of binocular space or the perception of form. We still take too much biophysics for granted. With the optical system of the eyes capable of forming clear images, with the lines of regard turning in certain directions conjugately, and with the retino-striatal systems in good condition, visual perceptions of various degrees of complexity are guaranteed. The sensory physiology which accompanied the older type of analytical psychology had to remain on this basis. I believe, however, that animal experimentation at least offers a new vista. For instance, the discrimination of brightness, size, pattern, and form is a peculiar complication of mental functions in the rat. With the human being, the corresponding visual analysis is done logically, by taking an aspect-attitude. With rats we can speak of different responses. We can now infer that their visual space is tridimensional. Their response to brightness-differences is not made to two flat surfaces. Brightness-difference seems to be basic to other discriminative responses. That being eliminated, they can also respond to size-differences but with difficulty. Various pattern-differences can be responded to after long series of training. Form as independent of pattern or brightness-distribution seems to be beyond their visual intelligence. Other aspects of perception which cannot be so easily described in human language are also effective, *e.g.* the trans-

posability from reacting to the smallest of three brightness areas to reacting to that card out of three which bears the largest number of brightness stripes, the total brightness of the cards being all equal. With the majority of the rats, the perception of size-differences and of rudimentary form-differences, both built up with difficulty and with special technique, seem to function only for a short while and then the animal relapses into other kinds of responses.[4]

The above summary does not represent all that we already know but the main lines of research have been indicated. The neural mechanisms for brightness, size, and pattern seem to be all different; that for rudimentary form perception has not been worked out. I have tried to isolate the visual areas from the other cortical areas without damaging the projectional fibers but have not been successful.

One cannot predict to what this line of research work will lead. That they are at least of supplementary value to sensory analysis and formal logic is beyond question. Epistemology is nowadays closely connected with physics. We need also a biology of knowing. Perception as well as meaning can be objectified.

The Comparative Psychology of Relation and Judgment

"Terms" and "propositions," "judgments" and other relations, these are more different verbally than introspectively. My mother tongue happens to belong to that group of languages which can make a syllogistic proposition without the copula and express "the feeling of *and*" without the "*and*" and sometimes "the feeling of *but*" without the "*but*." Just as we compare species of animals for the study of perception, so I venture to suggest that the comparative study of linguistic forms seems to be a fruitful approach to the higher mental processes. Up to the most recent times the psychology of thinking was built upon an unsound psychology of language. The latter cannot free itself from formal logic which exists in the shadow of the European languages.

It is true that one can approach human thinking processes in nonverbal experiments. One can, for instance, apply the technique for the study of animal "intelligence" or "insight" to human subjects. One

[4] Cf., *e.g.* J. R. Russell, Depth discrimination in the rat, *Ped. Sem.*, 40, 1932, 136-161; P. E. Fields, Studies in concept formation: I. The development of the concept of triangularity by the white rat, *Comp. Psychol. Monog.*, 9, 1932, (no. 2), 1-70; C. Y. Sun and C. W. Luh, Brightness reversal in form discrimination of the rat as compared with a young child, *Chinese J. Psychol.*, 1, 1936, 101-114; N. C. Shen, A study in the differential limen of 'size' discrimination by white rats, *ibid.*, 1, 1937, 191-220. Other data have not been reported.

may also extend the technique to include more complicated problems.[5] Human thinking, however, is usually and most conveniently done with words. This one can claim without reference to any theory of the relation between thought and language.

In common with all non-verbal experiments, linguistic psychology is interested more in the form than in the content of thinking. As a matter of fact very few psychologists have been concerned with linguistic forms as revealing thought forms. Like anthropologists, philosophers, free lancers, we sooner or later land on the stuff of thinking, elaborating on social or metaphysical norms. Comparisons between Oriental and Occidental thought are made in the most bizarre fashion which violate even the simplest rules of psychology and philology. We do not know what thinking is. Is it the same for the Chinese and for the Greek or the American? In China there have been within the last twenty years many books on the history of Chinese philosophy modeled exactly after the European textbook. Short biographical sketches are linked up with surmises about the universe and the meaning of life. Thoughts are compared in the same way as menus, with here and there a reference to differences in order, *e.g.* whether the minor premise or the birds' nest soup is first served. Nobody stops to consider how the Chinese make a proposition or judgment and what they take to be cogent, contingent, probable.

It will be a long while yet before the Chinese psychologist can describe in concise but understandable terms how he or his compatriot thinks differently from the Westerner or if he does so at all in a formal sense. It will be still more difficult to determine the cultural and racial significance of such differences. We have begun with some simple studies such as free association and the analysis of the language forms of nursery school children.[6] The experiments on generalization and abstraction which carry formal logic into the laboratory should be repeated in China and other countries in the Orient. Above all one must not be satisfied with mere references to the feelings of *and* and *but*. It would be far more profitable to compare their linguistic forms. For our purpose the feelings of *if*, of *therefore*, of *although*, etc., are really more significant. For instance, one may make a list of the various

[5] *E.g.*, N. R. F. Maier, Reasoning in Humans, *J. Comp. Psychol.*, 10, 1930, 115-143; 12, 1931, 181-194.

[6] The association-study, which I reported in 1932 in the now discontinued *Yenching Studies*, has led in one direction to a bookish analysis of what Stern and Piaget have called transductive thinking. Other inquiries are being pursued, the results of which I hope to report on in the future.

forms in which the Chinese introduce the *if* clause and compare them with Germanic or Latin forms. One may compare the psychological significance of those forms.

If the study of thought forms leads us into fields which the conventional psychologist is unwilling to follow, then we must remind ourselves of the old dictum that "psychology is what the psychologist bothers about." I have already indicated that we must be more outspokenly philosophical and technological. The same applies to studies of animal perception.

THE NATURE AND USES OF EXPERIMENT IN PSYCHOLOGY

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Editor, *The American Journal of Psychology*
(Founded in 1887)

Until the opening of our century psychologists stood in general agreement upon the means and the manner of experimenting with the human mind. For a generation *the new psychology* had been experimental. It came to America as it had been created and standardized in the German laboratories. It was experimental in the sense of physics and physiology. Just as the physicist assembled his apparatus to solve problems in light, heat, sound, and mechanics, and just as the physiologist arranged his saline bath, induction coil, ergograph and smoked drum for recording the activities of muscle, nerve, and blood-vessel, so did the psychologist proceed by similar means, to secure experimental evidence upon problems in sensation, attention, action, and memory. The logical formula was the same in all three cases. It was painstaking observation under control. Inasmuch as psychology thus borrowed from older sciences, it raised no responsible question upon the nature or the sanction of experiment and felt no hesitation in accepting the empirical issues of a method long considered sound and authentic. Rather did the prestige of the method promise to psychology a new dignity and a safe place among the sciences of nature.

But gradually in the early 1900s did great changes in interest and in envisagement appear in psychology, and never since those years have the character and the implications of experiment been so simply and so clearly defined as before.

The first complication in method arose from demands of education, biology, medicine, and business, which referred to psychology for quick solution many of their own problems, problems which seemed to call for easier, shorter, and more superficial methods than psychology had previously used. With education came the child and came testing, scoring and scaling; with biology came the historical doctrine of life and the description of animal behavior; with medicine came the clinic, the case-history, and the individual variant; with business came short-

cuts to the selection of employees and quick means of manufacture, advertising and sale.[1]

All these external influences brought a decided shift in the temper and the range of psychology, a shift which introduced many competing methods destined to make their vigorous way against experiment. Among the rival methods were tests, statistical devices, field observations, the superficialities of the clinic, the anamnesis, the puzzle-box and rat maze, the description of human and animal groups, and the study of such psychological products as the hive, the beaver dam, the nest, and the human city. Not only was the field of the psychologist enormously enlarged; it was also complicated by foreign interests and by imported modes of procedure. Experimentation was in part relinquished and in part adulterated to meet the new demands.

We speak of this event as the first Great Invasion into the field of psychology. Since it came from the sciences and arts of nature and of man, we may call it the *learned* invasion. One result of it was that the terms experiment and experimentation were given a wide variety of new meanings, whereas in the experimental psychology of the '80s and '90s the meaning was singular and definite. Among the plural meanings attaching today to experiment within psychology and about its immediate borders we can easily distinguish at least eight; (1) trying something, (2) making inquiries and seeking opinions, (3) carrying through a gross exercise to be translated into the gross conceptual jargon of some current doctrine or theory, (4) accepting a prejudice or proposing a theory and then finding persuasive instances, (5) watching a natural event and guessing at causes, (6) planning and arranging an event and also its conditions, (7) solving a scientific task with competence by the last-named procedure, and (8) submitting the results of this solution to a critical interpretation in the light of adequate and exact knowledge.

In all of these ways has the psychologist himself proceeded since the First Invasion under the sanction of the name 'experimental psychology.'

[1] Cf. *Essays on Research in the Social Sciences*, The Brookings Institution, 1931, 128. "Education brought to psychology many problems for solution; so did biology, and so did medicine. When appealed to, psychology was not ready. It was not prepared. But if it had been sufficiently interested in its own job and satisfied with its mission and its method, it would probably have gone its way and not been distracted. It was distracted; partly because it was not absorbed and partly because it contained a number of men who were primarily educators, biologists, or doctors; men who therefore sought for compromises, for a psychology which was pedagogical, biological, or medical. The younger men naturally followed, pushed by the pressure of the other subjects and pulled by the example of their elders."

Instances and illustrations of these variant procedures are not far to seek. The discovery of fact by (1)—'trying something,'—may be called the *haphazard* or *street* method of experimenting. The dyspeptic thus experiments with his health by the omission of the midday meal, by an hour of daily exercise, or by the glass of hot water drunk at dawn. Any such device *may* be therapeutic. The irritable sufferer is therefore willing to 'experiment.' (2) Having distributed convenient sheets of paper, the teacher asks for a favorite color, an estimation of the size of a dollar-bill, a 'social attitude,' or a belief in the masculine character of God. Again, he may 'experimentally' elicit folk-prejudices upon some matter of taste, fashion, or morals. The completion of the 'experiment' usually demands a statistical derivation of averages and mean variations, a coefficient of correlation, or a mathematical process of factoring. Numerical results are frequently taken as evidence of the 'scientific character' of the exercise. In the next form of experiment (3), words or figures may be uttered by the instructor and written down by members of the group, who are subsequently informed of their respective ratings in 'immediate memory' or 'range of attention.' A similar caricature of experiment is the alleged proof of telepathy by radio or a derivation of 'intelligence' by the same means. Here the resonance of psychological terms impresses the uninitiated. Once more, experimental proof by selective illustration (4) may take the form of the mind-reader's public demonstration or the breakfast recital to prove the 'fact' of dream-symbolism. Certain blackboard experiments by the advocates of *Gestalttheorie* fall in the same category. (5) The guessing at the course of a natural event is well illustrated by the method of free association where the experimenter declares that cerebral blocking or an inferiority-complex accounts for the response of the subject. When we add a deliberate setting and command of the conditions of the object or event to be investigated (6), we advance by a long step toward the classical definition of an experiment as observation under control; and when (7) we provide adequate training of the experimenter, we define still more adequately and rigidly. Between the formal control of (6) and the expert conduct of (7) a very great difference may lie. This difference requires a special comment.

Anyone familiar with the procedures of the drill or training laboratory in psychology, or in any other of the 'experimental' sciences, is aware that the student who follows directions, sets down readings, and draws inferences suggested by text, manual, or teacher often proceeds in a mechanical and perfunctory manner as under external instruc-

tion and fixed rule. Such a student may go through the postures and movements of experimenting without in the least himself deserving the name of experimenter, though he may be, in the implied sense, an *experimentalist*. It is unfortunately possible in some places to secure so high an honor as the doctorate in philosophy by the writing of a dissertation which goes little further than the mechanical solution, under imitative procedures, of a borrowed problem. The advance to our seventh variety of experiment is therefore notable, for this kind implies the trained experimenter who is himself responsible for the formulation of the scientific task to be carried through with just regard for relevant conditions and for the intelligent formulation of results. There is still wanting, however, the critical and responsible interpretation of the results (8) which adds the most difficult and most delicate of all the functions of the experimenter and which gives to experimentation its final sanction in the sciences because it incorporates the outcome of the procedure into a coherent body of attested knowledge.

These paradigms and instances are not the vagaries of imaginative construction. They actually correspond to a very great number of procedures in psychology which are called experimental.

No one who takes seriously and competently his psychological tasks will be willing, as we may assume, to accept all eight procedures as really adequate. Although every one of the paradigms may likewise be matched in the biological, the physical, the social, and the 'normative' sciences and although they all alike represent common methods of demonstration in everyday life, most of them fail to complete—in any scientific context—the essential character of true experimentation in the serious investigation of nature and of man.

If we agree to distinguish the merely mechanical, instrumental, and routine techniques from the truly and completely experimentational procedures, we shall have to be very exacting as regards the latter. We shall indeed have to ask for a task or problem clearly and significantly formulated, for adequate knowledge of the status of the problem and of relevant results previously acquired, for general and specific implications of the problem in a more general scientific context, for a command through training and experience of the situation to be created, for an expert apprehension of sources of error and of means for their elimination, and, finally, for the ability to complete a critical assessment of the results obtained.

Now it is of the first importance that we distinguish the *logic* from the *procedure* of experimentation. When we define experiment as observing under controlled conditions, we imply a logic of principles which distinguishes the method, on the one side, from casual inquiries and, on the other side, from all those means of understanding phenomenal systems and events by way of foregone conclusion, prejudice, rationalization, intuition, and the acceptance of magical agents and causes. But when we consider experimentation as a procedure, we describe the actual situation in which observation is to be made, the specific character of the materials to be experimented with, the nature and training of the experimenter, and the context to which the results are to be assimilated.

The early experimenters in psychology believed—as we have hinted—that the logic of experimentation was applicable in their own field as it had been shown to be in physics and physiology; a method which was presently to be carried over to the fields of embryology, genetics, geology, and many other subjects. This conviction that the characteristics and the operations of human experience, as well as of nature, were amenable to description by painstaking observation under control was corollary to the fixed belief of Western peoples in scientific method as a dependable and effective instrument of sound knowledge and effective research. The real test of the conviction came, however, only when the representatives of a new field of knowledge tried to adapt the method to their own subject-matter and to their own problems. It is one thing to observe under control the molar masses of the earth, another to experiment with chemical changes, still another to deal thus with living systems and operations, another again to follow through genetically the developmental stages of life or of the cosmos, and quite another thing to describe the behavior, the conduct, and the motives, thoughts, and desires of men.

Looking back upon the history of experimentation in psychology, it becomes apparent, as I think, that the pioneers placed an undue emphasis upon the formal concept of control to the neglect of the corollary concepts of subject-matter and individual training. There has, as I mean to say, been a tendency—possibly extending beyond psychology—to mechanize the processes of control and a corresponding neglect of the specific character of the thing or activity observed and of the expert qualities of the observer.

Let us examine in order these two neglects. First the neglect of the specific character of materials and objects of experimentation.

Very likely the difficulties of transferring a method from physics and physiology to psychology has, by this time, made the psychologist peculiarly sensitive to the important rôle played, in practice, by the stuff to be dealt with.[2] For some time, however, the great importance of this matter did not really emerge, for the early psychological stuff, as well as the pioneering experimenter, was strictly fashioned upon the physical-physiological pattern.

Since enduring things and functional products are, as a class, more easily observed and understood than processes and activities, it is natural that our first experimental essays should have regarded mind as a kind of existence paralleling body and should then have analyzed mind into mental units which paralleled, term for term, the more elementary parts of the somatic architecture (sense organ, nerve, and cerebral center). The sanction for this simplifying procedure was obviously supplied by a firm resolution among the sciences to analyze the world into final elements and, more specifically, by the principle of psychophysical parallelism. The search for elementary structures (sensations, feelings, conations, impressions, ideas, and the like) and for their bodily supporters naturally came, then, to engage many experimental psychologists.

Essentially the same pattern of experiment was subsequently followed by the behaviorists, only they—forsaking the mental things—replaced these by the world of external objects and translocated the parallelism to a fixed relation as assumed between 'stimulus' and 'response.' Here, once more, experiment prospered in the sense of the physicist and the physiologist but to the neglect of the organism regarded as an active and relatively independent living physical system and of certain of its operations.

The experimental search for ultimates of a mental sort brought to light an orderly array of colors, sounds, tastes, pressures, and other like objects specifically brought into view by a precise control of stimulating agents. To the structuralist these items were not qualifications of objects perceived or felt but mental processes. Thus was accumulated under exacting rules of observation a mass of factual materials which fortunately stands at our disposition quite apart

[2] The physicist, chemist, and physiologist are apt to appear somewhat bored by discussions of this sort. Possibly they consider that their professional ancestors have abundantly demonstrated the high value of experimental control in the conquest of nature.

from the assumptions of mental analyst or behaviorist, *i.e.* as divorced from the category of mental process or of response.

In their ardent search for strict parallelisms both older parties overlooked the limitations of their doctrines. The alleged parallels between stimulus and sensation and between stimulus and response are often modified by congeries of stimuli, by complications of bodily structure and operation, by context, by life-history, and by other circumstances. Certain protestant proposals, notably from the configurationists, have pointed out these limitations and have likewise suggested such concepts as the primacy of the whole (Gestalt) and constancy in color, size, and other aspects of objects in spite of variation in stimulating conditions. These proposals have, to be sure, not greatly improved experimental procedures, but they have helped to break down the old parallelisms and they have also suggested new problems.

The chief value of the factual results of the analyzing experiments, as matters stand today, lies in the relation which they do attest as between certain qualities in simple perceptive patterns, the basal activities of receptor and effector, and those stimulating energies outside the body (light, sound waves, chemical changes, etc.) which modify organic functions in physiological and psychological ways.

The same mode of mental analysis under experiment was also applied to the study of actions, as in the reaction-experiment. Both responses and reports were derived, the time elapsing between stimulation and movement being the significant aspect of response, sensations and images of the report. It was partly this study of action, taken along with a rising biological and human interest in the performance of rats, cats and dogs, which appealed to the behaviorists and gradually led to a bifurcation of procedure and interests. At present, however, any competent psychologist may use either procedure or both, according to his training and to the demands of a particular problem. Except for the satisfaction of espousing old causes and rival parties, the terms 'behaviorist' and 'introspectionist' have alike lost their significance and usefulness.

Another experimental variant developed in the earlier history was the psychophysical variety. Final analysis to 'elements' was again the goal; but here the results came, in time, to be interpreted as measures of capacity, invoking 'the law of error' and applying statistical means of dealing with large numbers. Thus were thousands of reports of 'likeness' or 'difference,' of 'presence' or 'absence,' submitted to a mathematical treatment in order to derive from painstaking reports

quantitative measures of inspective discrimination, of accuracy, delicacy, steadiness, and so on.[3]

Where the early analyses into mental elements came in time to promote our knowledge of receptoral activity and of the patterned side of the psychological functions, the psychophysical methods are now recognized as a useful tool in the interpretation at large of experimental results. Since it is presumably a part of the training of all younger psychologists to command the psychophysical, as well as the sensational, adjuncts to experimentation and also to learn the relevant facts of bodily resource, we may well dispense with another pair of terms, which now possess only historical significance; namely, the 'psychophysicist' and the 'physiological psychologist.' Who will not agree that the loss of four major but partisan terms marks an advance in psychology!

There are those who would counsel a fifth elimination—the title 'experimental psychologist.' That would have its advantage, if the limitation discouraged the use of the term for a separate kind or variety of psychology instead of simply denoting a fundamental method. This elimination would be difficult, of course, considering the varied interests, preferences and prejudices of man. Physics itself—most seasoned, at least by time—still distinguishes the experimental from the theoretical physicist. But there the distinction mainly denotes—as we may hope—a division of labor, not a division of the main subject. There experimental procedures seem more and more (barring occasional orgies of physical speculation) to be generally accepted as the fundamental means of deriving new facts and of proving or disproving hypothesis, dogma, and theory.

If we sought opinions from all who claim the title 'psychologist,' we should probably not obtain a unanimous voice for experiment. Some would say that 'experimental psychology' is old fashioned, others would elevate the nose of disdain. Many would prefer the seance, the divination of the analyst, the smell of the clinic, the sensational claims of the telepathist, or the magical free powers of statistics. Tens of thousands who find some 'new' psychology 'so fascinating and so human' will repeat James Thurber's patronizing comment (missing his fine irony) upon the dry and uninteresting facts of *normal* living.

That turn in psychology's affairs which we have called the Great

[3] The earlier contention that the psychophysical methods discovered *all the sensations* in a given series has little significance outside a particular party of sensationalists. A recent proposal to replace all psychological functions by the single act of *discriminating* seems likewise to be limited, though in another direction.

Invasion by education, biology, medicine, and business came just as that subject was on the point of advancing from its borrowing and adapting stage to the experimental conquest of a large region more specifically its own, the region of imagining, emoving, comprehending, planned action, thinking, and the developmental and social implications of life. Since these new projects and problems could not for a long time be really assimilated by psychology and since few of the demands of the immigrant subjects could be met—at least immediately—by the modes of experimental psychology already thrown into commission, it was inevitable that the adventitious methods which came with the invaders should have been promoted to the neglect of experiment. Mental elements and simple responses certainly were not in fashion among the invaders, who wanted to know how the organism became educated or deranged, adapted itself to the order of nature, and achieved efficiency in business and industry. Hence the decline of the older manner and hence the call for tests, scales, correlations, measures of habit, instinct, practice, intelligence, personal traits and attitudes, suggestibility, mental deficiency and psychological disorder. Hence the Babel of tongues and hence the origination of 'experiment' in all the imperfect and bastard senses of an earlier page. Experimental observation in the meaning of the more seasoned sciences was in danger of becoming a lost art and the highly trained and discriminating experimenter an extinct being. In the place of the latter appeared the tester, the clinician, the statistician, the question-and-answer man, and the artisan of practical employments. As experiment became thus confused, neglected and muddled, speculation grew amateurish, programs multiplied, and loose theories of personality, of general and special traits, of conscious and unconscious forces, of mental mechanisms and totalizing unities prospered.

In dwelling upon the need to distinguish the essential procedures from the logical form of experiment, we have described the part played by the character of the materials used and have touched upon the way in which point of view and theoretical setting influence problem, method, and outcome. Now we must regard the experimenter himself, for he is more essential to procedure than are materials and general envisagement. The real experimenter brings to his task more than a psittacoid knack of repeating rules and directions, of imitating a teacher, of setting controls, and of reading instruments and graphs. He brings training, aptitude, and a wide knowledge. We here distinguish the experimenter from the technician, a distinction important

to stress at the present time, for forms of procedure have become stereotyped, controls have become mechanized, and automatic means of registration and record vastly extended. The machine and the mechanician have, to be sure, diligently served man, his comfort, and his arts. The technician possessed of ample equipment may turn out results in a day which the experimenter could not formerly have managed in a year's time. It is only when the machine and the robot take the place, at the limits separating new from old knowledge, of the problem-solver and the thinker that the distinction becomes acute. The mechanical 'eye' and 'ear,' the 'magic brain,' and the devices of 'selection and choice' are so wonderful and so widely serviceable that men tend, even within the sciences, to minimize and confuse the function of the trained and creative experimenter, the real agent in that method which is called by his name.

Now if we were to stop short with the logical formula of 'control,' we might sensibly include all of those implements and devices which thus serve, and at times actually 'observe' (in the sense of this formula), the materials and the operations of nature, and thus overlook this most essential part of scientific procedure. To regain our balance we have only to consider the works and the accomplishments of great experimenters in the disciplines, the Faradays, Darwins, Helmholtzes, Pasteurs, Ludwigs, Curies, Stumpfs, Hertzes, J. J. Thomsons, Rayleighs and Michelsons, men who confirm our characterization of the really experimental procedure as carried through and interpreted by a wise, trained, critical, and creative individual, one who has the ability to set problems to nature, to create hypotheses, to devise new means of securing relevant solutions, and to recognize in his results an addition to, or a revision of, fact and theory.

Let us turn again to psychology in order that we may determine, if we can, the relevance of all this discussion of experimentation to methods and results in our own field and in our own times.

For many decades the prevailing psychology of Europe and America was an analytical psychology of consciousness. Its fundamental method (with the partial exception of England and France) came to be patterned—as we have noted—upon the models supplied by physics and physiology.[4]

[4] Most English psychologists continued to rely upon their native principle of association and France chiefly followed the clinical methods of the psychotherapists, with an infusion of philosophical and medical speculation. Although a few Italians went to school to Wundt and his followers, their country was more productive in anthropology, criminology, and certain branches of physiology than in psychology.

It would be a mistake, however, to imply that during this time psychology was not—especially during the eighties and nineties—preparing for its own métier, training men, and trying to re-shape its methods towards its own specific ends. Whether we examine the works of the great schools of Germany (Wundtian, Müllerian, and Stumpfian) or of America (structuralists and biologizing functionalists) we shall find a serious attempt to adapt experiment to new and peculiar problems and concepts of psychology.[5] Notably in the experimental literature of perception, action, attention, and the factors and results of memorizing, do we find good evidences of this attempt at assimilation and revision.

Some still believe that, had it not been for the Great Invasion, psychology would have consolidated its gains and remained close in spirit and methodology to the whole body of physical and biological science. Others see in retrospect sharp limitations and dangers in the older concepts and principles of psychology and now sustain the hope that, when the invaders have been really assimilated, the new stock will be found to be more healthy and more fertile than before. What the lineaments, the manners, the habits and the capabilities of the psychologist of 1950 are to be, however, only a confident prophet would try to forecast.

There can be no doubt that the uncomfortable process of assimilation is still in progress, with an unending increase in proposals, parties, interests, theories and doctrines.

The one real alarm from this process of adjustment to outside demands which disturbs the critically and historically minded arises from the circumstance that the frontiers of our subject now lie exposed to sentimental and private concerns of man—not merely exposed but inoculated, for the most recent invaders have come laden with, and infected by, these concerns. Symptoms appear in the cults of mental disease, in the popularization of dubious doctrines, in the revival of telepathy and other forms of personal magic, in an absorption in sex, crime, the privacies of the family, and neurotic behavior. In the

[5] For evidences of this statement it is wiser to examine the researches published in the journals than to rely upon textbooks and systematic treatises then current. 'Experimental psychology' has at times been made a synonym for 'laboratory psychology.' In *A History of Experimental Psychology* (1929), E. G. Boring so uses it (p. viii). Boring begins his *History* in Greek philosophy and finds his 'laboratory' well along in the second third of his book ("The founding of experimental psychology," p. 265). Many things have gone on in 'psychological laboratories' since Stumpf, James, and Wundt first used instruments in their studies in the '70s.

popular fancy all these things pertain to psychology. They are discussed upon the street, in casual social encounter, and by the lighter forms of literature, in a vernacular that is supposed to be psychological and with an assurance that the speaker or writer is familiar with the latest of all the 'new' psychologies.

No other serious discipline or learned subject is thus afflicted. So remote are the sciences of nature and of the cosmos from the private affairs, anxieties and desires of men that those itching infections are not, in these subjects, a constant menace to health.

While man's deep preoccupation with private, personal, economic and governmental affairs is in part perennial, it is now aggravated by the distress of the times; but we may well believe that its attachment to psychology is adventitious. It lies behind a second Great Invasion which, in all probability, never would have settled upon our subject had it not been for the first. After the impact from education, biology, medicine, and business, theory and doctrine were no longer counterbalanced by sound criticism and a clear-cut view of the field, hence the new sentimental and personal proposals were not generally met with vigor and resolved by the critical challenge which experiment inspires. It is as if a new furore for eugenical breeding were to invade biology, an ardent passion for making gold from lead and zinc were to possess the laboratories of physics and chemistry, or telepathy were to fascinate serious students of the herd or the swarm.[6] To be sure, many psychologists continue on their accustomed way and the records of their research help to fill the serious journals; but there is no denying the fact that human wish and social weather have their effect on man's sober efforts to understand, and the effect is especially strong when such innovations are exploited by literary writers and critics who carry to the great public new rumors, while at the same time they are unprepared by knowledge and training to discriminate that which is respectable and authentic from the tawdry and magical.

The results of this popular clamor are especially obvious in two places; in the bent and interest of the young and in the special psychologies, *i.e.* the studies of animal and child, of human socialization, of disorder, and of human types and varieties.

As many young as are not studious easily give themselves over to the psychology of the popular magazine and the daily paper and to

[6] Of course no one forgets the sad history of vital forces, animal spirits, n-rays, the Reichenbach *od*, ectoplasm, and other alleged emanations from living matter, nor the firm belief of many eminent men of science in a sixth sense, clairvoyance, telepathy, and communion with the dead.

uninformed writers on sex, crime, and neurosis, while the collegian either finds his private intake from these sources to suffice for his 'psychological' needs or else seeks out the less substantial courses in his curriculum.

As for the special subjects, the First Invasion had created a hybrid social psychology from sociology and biology, a spurious account of the disorders from psychiatry, a child psychology from educational theory and practice, and a doctrine of varieties from all these sources, with the uncritical addition of Jung, Kretschmer, and neighborhood types of personality. Then, before experimentation and other sound methods were able to counter, came the Second Invasion to substitute easy and absorbing narrations of disorder, childhood, sex and personality in terms of herding instinct, id, ego, libido, repression, conflict, the unconscious, and a lurid doctrine of lust in little children.

In its legitimate forms experimentation demands—as we have held—training, pains, accurate knowledge, and a critical spirit. It is natural, therefore, that men should abridge and mutilate it and should substitute for it many speculations, doctrines, and easy devices. Both the learned and the popular invasion have encouraged abridgment, mutilation, and easy substitution.

As we follow the later decades of experimental psychology, however, we must not overlook the fact that a number of the lesser methods which at first tended to displace experimentation have proved to be, in the hands of competent investigators, useful aids and adjuncts to it. Among them stand the methods of test, which have shown themselves to be subject to control and refinement and thus capable of a firmer interpretation than they receive in the hands of the amateur and the technician. The statistical methods of correlation and analysis, again, have been found to be as useful for establishing significant relations among the results of experiment as for denoting the degree of association among tested objects and abilities. The clinic, too, has been made, here and there, to yield useful results when steadied by true experimentation. Like its relatives, the questionary and the depictive case-history, clinical observation came into psychology either as an exercise of the technician or as a half-divinatory and casual bedside inquiry; but with improved use among the disturbed, the defective and the misbehaving (especially among children), the clinic has become in the hands of the competent a modified form of experiment which *faut mieux* serves descriptively, as well as diagnostically, in its field.

Prominent in this list stand the proposals of the behaviorist. The protestant reformation which rejected consciousness and causally related response to stimulus was initiated by ardent experimentalists who had concluded that 'a really scientific' method was impossible under the old régime, only physical energies and objects inciting the organism and only bodily behavior revealing human activity. Many problems yielded to the behavioristic procedures, which were, theory apart, quite as accessible to all other psychologists. The behaviorist's undertakings have succeeded best with embryonic forms, with relatively simple animals, and with animals readily trained to simple and stereotyped performance in maze and puzzle-box. Since these groups are all large and important for research, the output of this type of experimentation has been considerable and notable. Furthermore, many new physical devices and forms of control experimental psychology owes to this empirical movement.

In more complicated settings, however, the behaviorist has been less successful. Here he has tended either to ignore all those factors which lie beyond his SR formula or to revert illegitimately to such old forces and agents (many of them really 'mentalistic') as instinct, habit, memory, choice, lust, desire, purpose, and thought. Here the logic of control remains, but procedure and interpretation are often inappropriate or inconclusive. Fair instances are the proposal to deal 'experimentally' with language by counting words in the vocabulary-list and by displaying kymographic records of laryngeal movements, and to solve the central problems of thinking by reading encephalograms.

Another importation which has at once aided and disturbed experiment is statistics. First came the psychophysical methods, well designed to be delicate instruments of investigation. The meticulous determination of certain limits of performance turned out to be of less significance than the early psychophysicists hoped; but the methods have of late proved to be of great value in setting the experimental situation and in formulating experimental results over a very wide field. These methods are again notable because they accentuate the new demand for two trained individuals, experimenter and observer. This demand has both extended the field of psychological experimentation and thrown strong light upon the nature and the reliability of all trained observers in all the sciences of nature. The first brilliant demonstration of the psychological importance of the observing organism was the resolution of the 'personal equation' in astronomy and other physical sciences.

This notion of two trained individuals of unlike but correlative functions in the experiment may some time stand as one of the great advancements made by the sciences in this method. While it is peculiarly adapted to psychology, it would repay a fair trial in various biological and social studies. Those who have not acquired the respective trainings of *E* and *O* are wont to condemn this refinement by asserting that it implies 'introspection' and is therefore unreliable. It does not imply 'introspection;' it introduces no 'subjective' factor in the sense of mind, and it does assure, in competent hands, a measure of control and a revelation of facts not otherwise obtainable in many problems.[7] The limitation of the behavioristic form of experiment is chiefly due to the fact that its users insist that the observed organism be regarded as a 'subject,' taken in the sense of a *Versuchstier*, unable itself to observe or to report. The old desire to be 'as scientific as physics,' by aping the methodical formulas of that subject without considering the actual nature and requirements of psychological research, is very persistent.

Experiment under the trained experimenter is valid for a wide variety of problems touching the living organism—especially the socialized organism; and here the offices of experimenter are often (at times necessarily) divided between two associated individuals. The wider recognition of this fact should presently remove those caricatures of 'introspection' where the 'responding subject' is permitted to say casually only how he 'feels' or how he was 'surprised' or 'distracted'; and it should likewise inform the 'objective' psychologist that the relevant report of the trained observer calls, in principle, for nothing which any proper experimenter in any laboratory in any science does not demand of himself, *i.e.* the competent noting of matters-of-fact under appropriate instruction (formal, occasional, and self-prompted), together with a technical report attuned to the problem in hand (Fig. 1).

This splitting off of certain of the observational functions of the experimenter has a double sanction. The first is the sheer impossibility of directing, manipulating, and recording while *O* observes and reports;

[7] An early director of the Columbia Laboratory was wont to boast that his psychological problems were invariably solved without recourse to 'introspection,' a method whose import he seems never to have mastered. Though he stood apart from the more logical and clear-cut behaviorists, his experimental method was like theirs, save that he placed greater faith in statistical devices and was less careful of experimental controls. Conversion to the SR theory of animal activity at times followed indifferent success in the human laboratory; at other times, it seems to have rested on a primary interest in the engaging activities of the domesticated companions of man.

the second is that *O* is prepared to observe only when his organism has been stabilized by instruction and by the experimental situation, without those responsibilities which are properly reserved by *E*.

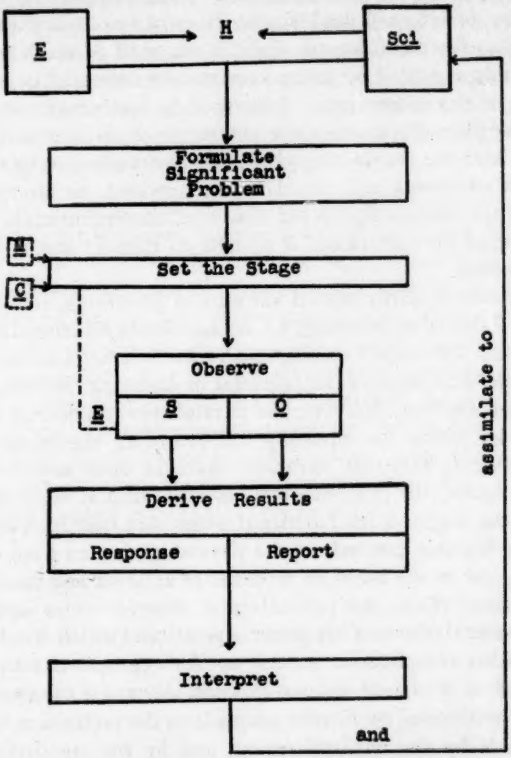

FIG. 1. EXPERIMENTAL PROCEDURES IN PSYCHOLOGY.

E = experimenter
H = hypothesis, conjecture
M = mechanic
C = conductor
S = subject
O = observer
Sci = body of facts and principles

(The more adventitious aspects of the method are indicated by broken lines.)

An important question remains: What and how does *O* report? *O* reports first the outcome or issue of his psychological operations, *e.g.*

the character of the object or event perceived, remembered or imagined; differences, similarities, and the like, secured by inspecting; the results of acting (plans, resolves, doubts, new body-objects, etc.), of comprehending (topics, explanations, and intimations), of thinking ('thoughts,' beliefs, and doubts), the issues of search, and the resolutions of emotive predicaments. Such is the stuff of much reporting, unfortunately regarded by many as somehow 'mental,' or as lodged in 'mind' or the unconscious.' Some of these issues may—it is true—be inferred from the spontaneous comments of an untrained subject as well as from the graphic record of instruments actuated by the body. Since such comments and records must, however, be interpreted by *E* before they become significant results of experimentation, it is far better method for the trained *O* himself to identify and report by a fixed procedure.

The observer secondly reports varieties of perceiving, remembering, emoving, elaborative thinking, *i.e.* he announces functional events as they proceed. This report corresponds to the first-hand account of the physiologist who inspects the processes of digesting, secreting, transporting, and the like. But since the physiologist has neither training, concepts, nor terms for reporting and recording the psychologist's list of functions, this task naturally devolves upon another sort of observer. Again, the psychological observer familiar with the character and the origin of his functional issues may infer backward from product to function precisely as the physiologist infers from the presence of oxygen in the blood to processes of aeration and fixation.

In the third place, the psychological observer notes and reports upon the general course of his several operations (which would appear in bewildering complication were it not for appropriate control) and upon factors of initiation and government. Always is the character of the report determined by *E*, who adapts it to the problem in hand and guarantees it by the physical setting and by the stabilization and attunement of *O*.[8]

Since the human organism is a relatively independent physical sys-

[8] A significant similarity to the procedures (not to the theory) of conditioning is apparent. In these procedures the organism is first stabilized as to general physiological state (sheep or dog accustomed to harness, bodily instruments, surroundings, companions, human operators, and so on) in order that the clean and specific effect of a new physiological integration (as from buzzer and metronome) may be observed and recorded. Thus are avoided the artifices of excised preparation, surgical insult, diseased tissue, dissection, etc., a device of method which corresponds to the older psychologists' abstraction from the total living organism of a mental element or the isolated response of a single muscle-group, and which suggests the subtler stabilization of the trained *O*.

tem, containing many intrinsic variables, attuned by a long biographical and biological past, and capable of reaching a given functional end by alternative means, the assertion that simple control of stimulus (an impinging energy) meets the requirements of real experimentation is warranted only in such simple problems as those previously distinguished. In such operations as the psychologist seeks to describe, a far more refined plan of experimentation is usually required, a plan which necessitates—unless psychology is unduly to limit its task or to satisfy itself by satisfying its importunate invaders—a form of experimentation adapted to its own specific uses.

Should we agree to refer the present troubled state of psychology to the two Great Invasions, we might still properly ask 'Why the invasions?' Is it a great drought that has brought the animals, pushing, roaring, snarling, and competing, to the water-hole for quenching and relief? Only an African zoo, spontaneously assembled by thirst from the veldt and jungle, could equal our present clamorous variety. Possibly the thinning, selective and disciplinary processes of time will clarify the scene. They will if they do but relieve the psychologist of extraneous and distracting interruptions and permit him time and a quiet occasion for formulating anew his main point of view and for incorporating all those useful adjuncts and extensions of the new century into a sound experimental program.

TITCHENER AND THE EXISTENTIAL

By Edwin G. Boring, Harvard University
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(Founded in 1887)

Titchener's defense of the existential mental datum, his convictions about it, his deep faith in it, deserve to be classic in the history of modern systematic psychology, and indeed are likely to be if all the facts can be got into the record. His position ought to be classic because it represents the culmination of the trend of content psychology that began distinctly with Wundt and achieved its destiny with Titchener. And it ought to have been Titchener who represented the trend, because he was clearer than Wundt and beyond him, because he was more systematically explicit in his later years than even the systematic Müller, because unlike Külpe he never gave quarter to the Brentano school of act, because he went further in these systematic matters than other Wundtians like Meumann, or Brentano men like Stumpf, or free lances like Ebbinghaus, and because the new generation in Germany has trafficked with what in America we have tended to call behaviorism.

Titchener was pivotal. He was the best and the last of that school, not only in America, but in the world. How far we have come away from his views in the brief ten years since his death was borne in upon me when I was reading some of his letters. They took me back to a strange world once familiar. These issues, so all-important to the group of us who derived intellectual sustenance from Titchener, what were they? *Introspection*, *dependent experience*, *sensation*, *attributes*, *dimensions*, *integration*, *perception*, the *stimulus-error*, *meaning*—above all, *meaning*. All these terms are still current. The new graduate students know them. But what graduate student of the 1930's could supply for these foundation stones of that psychological edifice the descriptions current in the 1920's at Cornell and Clark and Harvard and Princeton and even Yale and Pennsylvania, all those places where Titchener kept the spark of 'truth' alive by his annual progress to the spring meeting of the 'Experimentalists'? I doubt greatly that the modern graduate student could even understand these letters of Titchener's on systematic issues; and certainly he could not expound them.

The gross facts of Titchener's axioms and his formulae for *introspection* and *mental process* are readily recoverable from his writings. Anyone can look them up. Here I want to describe the atmosphere of the 1920's when the phenomenology of the perceptual integrations of sensory dimensions had replaced the older elementistic conceptualism. It is the deadly seriousness of the *mental* as Titchener distinguished it from the *physical* that already begins to seem to us faintly impossible. For this purpose it is right for me to quote from Titchener's letters, letters written informally, casually and personally, without thought of a wide public and often directed toward the didactic correction of my own heresies. Private correspondence such as this must be privileged to escape criticism, but I venture to suppose that it may fairly be employed to furnish substance to an atmosphere, to indicate the temper of a climate.

First let us recapitulate some of the chief points of development in Titchener's systematic views, the items that the psychologists born before 1900 already know. In 1910 Titchener was defining mind "as the sum-total of human experience considered as dependent upon the experiencing person," or "as dependent upon a nervous system. And since human experience is always process, occurrence, and the dependent aspect of human experience is its mental aspect, we may say, more shortly, that mind is the sum-total of mental processes."[1] That conception Titchener derived from Avenarius' formula.[2] The difference between physics and psychology lies in the point of view from which each regards experience, for "it is the same experience all through; physics and psychology deal with the same stuff, the same material; the sciences are separated simply—and sufficiently—by their point of view."[3] This differentiation of the sciences by point of view and not by subject-matter Titchener reaffirmed about 1919: "All the sciences have the same primordial subject-matter, which they approach from definitely different points of view, and . . . their developed subject-matters diverge because and only because these different standpoints afford different readings of existential experience."[4]

All this seems clear, but there is nevertheless a paradox. (a) All sciences have the same primordial subject-matter: *experience*. (b) Every science has its own developed subject-matter: *the aspect of experience* that

[1] E. B. Titchener, *A Text-book of Psychology*, 1909, 16.
[2] Cf. Boring, *A History of Experimental Psychology*, 1929, 390-392, 410.
[3] Titchener, *Text-book*, 8.
[4] Titchener, *Systematic Psychology: Prolegomena*, 1929, 268 f. Weld says that this part was written in 1919.

it views from its peculiar standpoint. The distinction of the sciences by point of view implies the existence of this primordial experience that is never realized in itself, but appears only as this and that scientific aspect. One needs to be aware of this distinction if he is to understand Titchener. Titchener never published anything incompatible with the statement that the sciences have the same subject-matter and differ only in their points of view, but there is only a hair's breadth of distinction between these two conceptions, and informally Titchener talked and wrote as if psychology and physics had different subject-matters. At least that is the easiest interpretation of his "a world of matter and a world of mind."[5] Moreover, we shall see that the world of mind was very real to Titchener, a world in which a man could live and think and observe and grow wise. It is to remind ourselves that there was such a world for the psychologist not more than a decade ago that I undertake this exposition.

The other difficulty in the understanding of Titchener's system concerns the terms of description. Titchener, following Wundt, originally thought of these terms as elements, although he avoided the error of substantialization by noting that the subject-matter of psychology, like that of physics, is always "process, occurrence," "a stream, a perpetual flux, and not a collection of unchanging objects."[6] The most certainly correct example of the mental process was the senation, and the sensation, the elementary term of description, was defined, as Külpe had said, by its attributes. That was Titchener in 1908 and 1910. He accepted Külpe's criteria: (1) the attributes are inseparable, for you can not abolish one without abolishing the sensation; and (2) the attributes are independently variable, for only thus can they ever have been differentiated from one another. On the other hand, Titchener increased the number of attributes from two to five, adding clearness (in order to describe attention), duration and extensity to the Wundtian pair, intensity and quality.

No difficulty appears until Külpe's criteria are combined with Titchener's insistence on the processful nature of sensation. If a sensation is a process, it must be changing; but, if it is changing, then in respect of *what* is it that it changes? If it changes in one of its attributes, then it is no longer the same sensation, provided Külpe's definition holds. And, since its attributes are its all, there is nothing else in respect of which the sensation can change. In 1913 Rahn brought this criticism

[5] Titchener, *A Beginner's Psychology*, 1915, 9-12.
[6] Titchener, *Text-book*, 16.

and others to bear against Titchener's system,[7] and in 1915 Titchener shifted ground.[8] The attribute became for him the immediate object of observation and the sensation a systematic classificatory term. Titchener did not admit a change of view here, yet this paper began the period where the attribute became more important than the sensation. The sensation had been relegated, as it were, to the "applied logic" of systematic psychology. The attribute as the datum of observation became separable in observation and was thus free of Külpe's criterion of inseparability. Therefore the noun *sensation* seemed to lose importance at Cornell, although the adjective *sensory* in the sense of pertaining to the attributes was frequent.

Now an attribute needs to be *of* something, and, with the sensation lost, there was nothing left for the attributes to be *of*. Titchener, who always took physics as the model for psychology, had often spoken of the fact that all of physics is expressible in terms of three dimensions: space, mass and time (the c.g.s.-system). It must have been soon after 1920 that he began to use the word *dimension* instead of *attribute*. The term appears first in my letters in 1922. He printed a note in 1924 about the five dimensions: quality, intensity, extensity, protensity (for duration), attensity (for clearness, vividness).[9]

There is a question as to how Titchener regarded the dimensions. A study of his letters convinces me that he thought of them as actual data of observation, and not as systematic constructs, frames of reference in respect of which experience can vary. The fact that in 1915 he contrasted *sensation* as a systematic term with *attribute* as an observational datum supports this view. The fact that the Cornell theses of the 1920's imply that mind or perception is a dimensional integration still further establishes my belief that Titchener thought of the attribute as a datum.[10] Titchener belonged to the age of positivistic science where to question the availability of experience to observation was to question the possibility of science. He never lost faith in the existence of an observational datum, and that is the significance of the word *existential*

[7] C. Rahn, The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, (no. 67), 1-131.

[8] Titchener, Sensation and system, this JOURNAL, 26, 1915, 258-267. See foot-note 34 *infra*, and the text to which it refers, in order to understand why Titchener thought he had *not* shifted ground.

[9] Titchener, The term 'attensity,' this JOURNAL, 35, 1924, 156.

[10] Cf. Boring, *The Physical Dimensions of Consciousness*, 1933, vii, 20-22, 32 f. But my study of Titchener's letters has changed my mind since 1933. I now think that Titchener meant by a *dimension* an observational datum, and that the notion of a dimension as a frame of reference for a mode of variation and thus as a systematic concept is my own contribution to this theoretical development.

in a discussion of Titchener's views. His system has even been called "existential psychology."[11] It is plain that Titchener thought of space, mass and time as physical data, and not as concepts or constructs. The neo-positivism of operationism was unknown to him. Space, time, quality, intensity and even attensity were real to Titchener as data. They stood up for him under observation. They were the contents of the world in which he as a psychologist lived, a world that has nevertheless gone out of style.

Psychological observation. Titchener's faith in observation as the chief scientific method was never daunted. Scientific observation, he thought, requires a trained observer, since experience has to be viewed from a particular standpoint. Thus he admired Purkinje as the skilled, dispassionate, accurate observer. He preferred Hering to Helmholtz. He had seen the advance of scientific psychology so often thwarted by the philosophical interest of psychologists that he was ill disposed to philosophy within psychology. He spoke of the systematic issues in psychology as "applied logic," and, although they were sometimes his primary concerns, he often slighted them. On the primacy of observation, he wrote:

> Science in the living is a tight-rope walk with observation on the one hand and applied logic on the other. If you must fall, then by all means fall toward observation; you will henceforth be defective, but you will live. Helmholtz is the sublime example. If you fall toward logic you are killed.[12]

Of good and bad observation at the beginning of science, he said:

> There were observations in experimental psychology before there was a logically definable experimental psychology, just as there were moments of scientific attitude before there was a definable scientific attitude. But the observations were, precisely, muddy and the attitude casual. That is to say, the living of science was neither pure nor sustained.[13]

Titchener could commend even the philosophical Wundt because he added continually to psychology's "store of observations:"

> Wundt undoubtedly 'made' experimental psychology. He named it: that is, he saw its field. He experimented and founded a special laboratory: that is, he made academic chairs henceforth possible and added continually to the store of observations from which the professors should teach. He had a critical judgment against the past: that is, he gave us an historical background from which also we might teach. He had so much of completed

[11] Cf. Titchener, "The one complete and positive reply to intentionalism is the existential system, the system that is partially and confusedly set forth . . . in the works of Wundt and Külpe and Ebbinghaus," *Systematic Psychology: Prolegomena*, 1929, 256. See also H. P. Weld, *Psychology as Science*, 1928, 61-68.

[12] October 1, 1921.

[13] October 1, 1921.

system as the time allowed: he never fell short, though in the light of events we can see that he was often overhasty with his logic. Above all, he was temperamentally through and through psychological, and I believe that he is the first man of that type to appear in the histories of philosophy. Nobody but a psychologist could have 'made' experimental psychology: James is the instance; he had the chance and let it slip. Finally, we could go on working now for 20 years within the limits of the PP [*Physiologische Psychologie*], and we should still be advancing; though no doubt we shall advance more quickly as we are.[14]

Titchener believed that psychological observation is favored by expert familiarity. If you live in the world of odors for a few years, you may then know them well enough to classify them correctly. The following comment of Titchener's on odors shows how his students could interpret him as saying that psychology has its own peculiar subject-matter.

I think the trouble with all three of these papers is that the observers have not lived themselves psychologically into the world of odours. Whenever I test the Henning prism in the case of isolated odours with which I have been long and intimately familiar, I find confirmation; but one can't, unfortunately, take the time for systematic observation. . . . If one could get a couple of observers who were naturally interested in odours and who were willing to devote three or four years to intermittent observation, then I still believe that the Henning prism would come out. You know yourself very well, by way of your work on pressures and temperatures, how absurdly unreliable are the judgments of perfectly competent persons who have had general but not special training.[15]

A year later Titchener was expounding the comparable observational doctrine for touch. He was discussing the tactual continua of his proposed touch pyramid.[16]

I am not at all concerned about the marked qualities; anybody can get them who is willing to gain some familiarity with touch and then to repeat certain classical experiments; what I am concerned about, and what was new in the pyramid, is the establishment of qualitative series. The trouble here is the old trouble of touch in general—that you can not demonstrate or give positive rules for experimental procedures which shall insure demonstration as you can in the case of sight. You have just to live with the qualities over a term of years; noting anything that happens in every-day life, and noting also and especially anything incidental that crops up in the course of regular experimentation. Then you find series are arranging themselves in your experience—or rather, I ought not to say series in the technical sense—but rather sets or arrays offer themselves in arrangement. Then, of course, one tries to put these sets or arrays together, tries to find terminals, tries to attain qualitative series proper. I am not sure that I have done all this; but I am very positively sure that qualitative series are there. If I were working by arm-chair construction (which means, I suppose, that one scrapes together the various

[14] October 1, 1921.
[15] March 4, 1924.
[16] Titchener, Models for the demonstration of sensory qualities, this JOURNAL, 31, 1920, 213 f.

names occurring in the literature, as Zwaardemaker did for odours), then I could very easily have worked in warm and cold, which I now exclude. In effect, it is pretty much the same question that comes up as regards the smell prism; you must either trust the expert or you must yourself take the problem seriously and so control the expert's results by your own.[17]

The same faith in the scientific adequacy of intimate observational acquaintance appears in a letter of Titchener's which gives directions as to how to find the bright pressure that is associated with pleasantness and the dull pressure that is associated with unpleasantness. This letter was written only three months before his death.

(1) It is not the least good starting to look out for the pressures; if you try to compel them in any way, you will not get them. Nor is it any good to try to shift attitudes at the very start; one has to learn to walk before one can do gymnastics.

(2) The thing to do is to take some stimulus—I recommend an odour—which you know by experience will probably arouse in you a weak P or a weak U. Sit down to it, and see what happens. If you get nothing but the odour quality, the experiment has failed. If the stimulus was affective, you will find in addition to the odour quality and mixed all up with it either something buoyant, light, flimsy, filmy, diffuse, or else something heavy, dull, thick, oppressive. The blend of odour quality and this additional something fills the whole field of experience in the observational attitude; there is nothing else; there is no trace of perception; and the odour quality itself is likely to be more or less lost for identification in its accompaniment.

(3) When you have once put your finger on this addition to the odour quality, it is only a matter of time and repeated observation to realise that its own quality is that of pressure. A good many trials may be needed; but the outcome is quite sure.

(4) When the addition has been recognized as pressure, or at any rate as pressury, then one may begin to try the shift of attitude. One notes, at the presentation of stimulus, that the odour is weakly pleasant or unpleasant; then, having made that note, one changes from the perceptive attitude to the observational, and lets oneself float off, as it were, in the blended experience. There is a knack here which must be learned by actual trial.[18]

Attributive relationships. During the last seven years of his life Titchener was much interested in the interrelations of the attributes. In 1920 he was writing letters about how to mix Warm and Cold with Heat in qualitative series, after the analogy of the mixture of Black and White with Gray. The touch pyramid of 1925 (*vide supra*) was one result of this line of thought. In 1922 he was concerned with the visual attributes and the intensitive nature of chroma (saturation). The notion of the color figure as a solid set of intensitive radii emanating from the central gray was the result.[19] In 1924 he was writing letters about the

[17] October 23, 1925.
[18] May 4, 1927.
[19] Titchener, Visual intensity, this JOURNAL, 34, 1923, 310 f.

tonal attributes, the relation of tonal volume to tonal localization, the nature of intensity, and the question as to whether pitch is truly qualitative. The general problem of the nature of quality was also disturbing him. Since these last two matters give some indication of what Titchener hoped to do toward a psychology of sensory dimensions I quote concerning them.

I can't for a moment bring myself to believe that pitch is quality. Volume, of course, is not qualitative. But pitch I take to be a partly common-sense and partly physical term analogous more or less to brightness in vision; as we can run up the scale of brightness from black to white in a straight line, although qualitatively and psychologically we make a turn of 180 degrees at grey, so more or less can we run up from the lowest bass to the highest treble in a straight line of pitch. Quality, no doubt, is involved, as it is in the visual brightness series; but pitch is not quality, I take it, and how quality is actually involved in pitch we simply do not know.

I don't either think that vocality or tonality is quality. Quality I think will turn out to be the 'body' of a tone; and this 'body' will involve pitch and will involve also something more or less like vocality. I haven't any further least idea on the subject, but I am quite confidently telling my sophomores these days that we don't yet know psychologically what the quality of audition is. Somehow music and speech comprise the thing, but somehow or other they also manage to hide it.[20]

Two weeks later Titchener felt that he had advanced in his understanding of these matters. He had reasoned his way into a possible interpretation of the nature of octave recurrence in the tonal series, in hopes that the tonal variable that gives the octave relation might turn out to be 'true' tonal quality. That is why he insisted that "pitch is not quality." His long letter on this subject, however, gives us little more information as to what would make quality 'true,' and I forbear from quotation.

Again in this context of the nature of the attributes Titchener championed "living with the experience for a term of years" as the "magnificent" method.

One may use a generally determined observation, in which one directs the observers to observe quality simply, and the judgements of comparison are given in terms of like and unlike. Here the guiding thread for the experimenter must be the qualitative series. I suppose that Henning worked on odours in this sort of way. If one can't have recourse to qualitative series one must try some other methodological trick, as Nafe did in his study of the affective qualities. I suppose that Koehler started out in this way when he hit upon his rubric of vocality. . . . The . . . general method is magnificent if one can take the time to live with the experiences for a term of years, and if during this period one can strip oneself of all biases and prepossessions. Unluckily the auditory atmosphere seems to be loaded with biases and prepossessions; vocality gives us the horrible example.[21]

[20] October 22, 1924.
[21] November 11, 1924.

It seems clear that Titchener hoped that an understanding of the psychological dimensions would lead to correct theories of sensation. First he thought we needed to get at the "real" dimensions. He doubted pitch as the real tonal quality. Here is his doubt about the validity of intensity.

Perhaps, in order to expose all my incubating heresies, I ought to add that during the past few years I have been entertaining mild doubts regarding the reality of the dimension of intensity. Of all the dimensions that I now recognize—quality, intensity, protensity, extensity, and attensity,—if I were to be forced to say which I regard the shakiest and least well established, I should answer intensity. Here again, I have nothing at all that is ripe for printed discussion; you know yourself how it is, that an accumulating number of tiny indications gradually get possession of one, until a cranky idea formulates itself, and then the idea sticks and takes on more and more the colour of possibility, until presently one almost finds oneself regarding it as probable.[22]

Ultimately he hoped that it might be possible to achieve a general theory of quality for all the senses.

I have sometimes dallied with the idea that quality too must ultimately have a generalised psychological theory, identical in principle for all the modalities. I think we have at some time in the past discussed the possibility of bringing all the modal qualities together; at any rate I have long thought that even if all the psychological qualities do not form a continuum, they may form a sort of hinged or interconnected total—so that, to put it crudely, the colour pyramid somewhere hooks on to the smell prism, and that to the taste pyramid, and so forth.[23] I admit that the evidences of such a passage are very few and very obscure; still one can't help playing with the idea. In this case the theory of auditory quality ought to go on all fours with a theory of visual quality or a theory of touch quality or what not.[24]

Dimensions. Titchener's first use of the word *dimension* in his letters to me was in 1922:

I have read a little more of McDougall, and I am quite sure that his own thinking goes on in associational terms. Where I should operate with dimensional integrations he is still working with clusters of sensations or clusters of images. I wonder what he would say to f = ma? He would have to allow that the physicist, in talking of mass acceleration, has neglected all the motive, purpose, significance, of the word force. But I doubt if the physicist would find his withers wrung.[25]

One sees here that Titchener had the physical dimensions in mind. In writing "f = ma" he was thinking doubtless that $f = ms/t^2$ in expressing force in mass-space-time units.

There had, however, been talk about "dimensions" and the next spring I sought to use the word in a MS which I sent to Titchener for criticism. He objected:

[22] November 19, 1924.
[23] Titchener added in the margin: "Too crude! Not at the corners, of course."
[24] November 19, 1924.
[25] October 25, 1922.

The word dimension itself has not been published, and I think it should not be without a justifying and explanatory discussion.[26]

My paper was the note about Yokoyama's study of the observation of attributes,[27] and we were still arguing about the matter the next fall, with my paper unpublished. Titchener said that my note "crossed his position" and he wanted agreement in advance of publication. We compromised and, among other things, I did not print the preëmpted word *dimension*.

In 1924 Titchener used the word *dimension* in his note on attensity.[28] It is the following paragraph, taken together with Titchener's insistence that there is a world of psychological things like odors and tones and sights with which one can gain intimacy by living alertly in it, it is these two facts that convince me that Titchener meant by a *dimension* an object of psychological observation and not a logical frame of reference for a mode of variation.

I believe, of course, that any psychological dimension may be observed, of and by itself, over its whole range of variation. Whether the variation of one dimension is correlated with variation of another dimension, when both are in the field of observation together, is simply a question of fact. Whatever happens of this sort, positive or negative, I assume will be reflected in the correlated biological events.[29]

I think Titchener believed that a quality is given as such to observation at the observational instant. The mode of variation of the quality would have been for him the qualitative series, and it would thus be given only in many successive observations. We know that Külpe's criterion was based upon the fact that it is not possible observationally to distinguish one attribute from another unless the attribute in question varies independently of the others. The "dimension" may seem to be this mode of variation, this experienced continuum, which is essential to the recognition of the attribute. I can not say what Titchener would have said had he been asked how the attribute or dimension, known only by its variation, could be recognized in itself as a particular value without variation, but I hazard the guess that he would have said that variation is necessary to observational training and that, given the training, the observer can then recognize the particular dimensional datum by itself.

Tactics. Titchener took his psychology aggressively. He entrenched himself over against the American majority and waged constant war-

[26] March 22, 1923.
[27] Boring, Attribute and sensation, this JOURNAL, 35, 1924, 301-304.
[28] Titchener, The term 'attensity,' this JOURNAL, 35, 1924, 156.
[29] November 11, 1924.

fare against it. His lines of communication ran directly to Germany, to Wundt and Müller, and, less importantly, to Stumpf and Brentano. The American front was definitely established. The Clark of Sanford and Baird belonged with Titchener. Baldwin and Cattell commanded the enemy. The Experimental Psychologists was Titchener's answer to the menace of the American Psychological Association. In this atmosphere publication became something more than clear exposition; every paper was a manoeuvre. Titchener wanted to be effectively convincing and for that he needed to choose the time and manner of his every saying. It was not enough for him to be clear and right—human perversity being what it is.

This attitude of Titchener's is illustrated by his discussion of his difference of view with me about Yokoyama's research (cf. *supra*). Titchener wanted me to adjust my view to his before I published.

> You have been so generously good about this paper that I feel some compunction about my first critical objections; perhaps it would have been more 'scientific' to let you publish uncriticised, and then to have taken my own chance publicly with you in rejoinder. My motive was the feeling that it is rather fatuous for you and me to get into public controversy, since you are bearing the brunt of what is at bottom an attack on both of us: and so if I chip in and you are between two fires the common enemy would rejoice. My only reason for chipping in is, of course, to safeguard my (which is also largely your own) position; and the need for this safeguarding arises from your tendency (as I view it) to give too much ground to the enemy at the start,—or, in another simile, to blunt unnecessarily the weapons that I and your own work have put into your hands. If then I can persuade you to make such minor changes as leave me free for further tactics after you, that is (it seems to me) so much clear gain. If on the other hand I have to disavow your tactics after you have fought your own engagement, the enemy would surely take new heart—no matter how badly he might have fared for the moment at your single hands.[30]

Later in the same letter Titchener discussed and defended his tactics as against mine.

> I can see your plan of present offensive. It is clever, and it may bring results. It reminds me of Dumas' caution: il serait bon de s'en tenir, autant que possible, à un dogmatisme largement objectif. But I can't see that it doesn't involve, all along the line, logical equivocation of terms. When you ascribe an identical phrase to Cattell and (truly on your own authority, by hypothesis) to Mach, I think: Yes, but if the two men had said that, they would have meant very different things. You have, I know, a sort of passionate desire to come to close quarters, to get to grips and let the better man win in the tussle—I mean, the man with the better position (not the cleverer man). So have I. Remember that I am the first man in the world (so far as I know) to defy Brentano in the open after having read him with sympathy (now, of course, every half-fledged talks of Brentano as if he understood him). I have been trying for a generation to get to grips. Well then: ***whenever I have made methodical concession, I have presently suffered for it***, and have had to retake the

[30] September 24, 1923.

ground by open warfare later. That will be your fate also. You are also very desirous of being understood by the others. So have I been; have I ever written obscurely or without pains? But I have failed; and so will you. Have Kantor and Kelley understood you? So when you say that the enemy interests you and does not interest me, you are not quite on the track. I am still interested; but I think it is useless to traffic with the enemy *on his terms:* I prefer to go on preaching the gospel for him to take and be saved or leave and be damned.[31]

Fluid concepts. No aspersion can be directed against Titchener's sincerity in these tactics of exposition. He 'knew' that he was right; he knew that he was bound to be misunderstood; and he did the best he could to circumvent the forces that would have delayed success. Moreover, he was also concerned with the enemy from within, the problem of the psychologist's own past commitments which prevent the progress of his thought. A man needs, he said, to stay master of his own concepts in order to use them effectively for himself or against the enemy.

So Titchener argued that concepts should be kept fluid, and he took me to task for hardening mine. The following paragraph is part of his criticism of my paper about the stimulus-error.[32]

So I thought the R-error paper a jolly good paper. But I thought many other things in detail. I thought your apologetic setting at the beginning was distorted in perspective; I thought that you were wrong on sundry points of detail; above all I deplored the visual schema and its elaboration in words—because for me a visual schema is a very good thing to have in your head but a dambad thing to commit yourself to on paper. I believe that you will have years and years of trouble from that diagram—your 'funeral,' of course; and you are grown up and can afford to pay the undertaker. And so on. What I thought in generalest general (and I have a memory that I said this!) is that you tend both to harden concepts and to slight definitions (your own concepts and other people's definitions). Here we differ temperamentally: I like to stay master of my concepts, and I have a sort of morbid scrupulosity about the meaning that other men have given their technical terms. I think that I said this to you, at the time, and I should have said it the more readily as I recognise the temperamental difference that underlies the literary difference.[33]

To this letter I replied that I found Titchener's fluid concepts indefinite and therefore misleading, and that I had been seeking to harden his while he sought to soften mine. His rejoinder is worth quoting *in extenso* because it puts the matter of hardness and fluidity in conceptual thinking so clearly, adding Titchener's comments about himself and Wundt as illustrations.

I have only one general comment to make upon your present letter, and that is that to my mind the fluid concept is always unequivocal and the hardened concept always

[31] September 24, 1923.
[32] Boring, The stimulus-error, this JOURNAL, 32, 1921, 449-471.
[33] September 24, 1923.

equivocal; just the very reverse, I think, of your own interpretation. I should say, for instance, that Wundt had failed of the impression that he really deserves to have made largely by reason of the fact that he hardened his concepts too early, and that therefore they were all equivocal. He tried to put a definite stamp upon consciousness and upon apperception; the consequence was that the terms shifted in meaning from book to book without his being able to see the shift and therefore to make acknowledgement of it; and the net result is, I take it, that both of those terms, which might have been extraordinarily useful, are permanently lost to experimental psychology. If he had kept both concepts fluid from the beginning, we should find ourselves using them freely and usefully to-day.

As a counter instance of a concept that has been kept fluid for practically a generation let me take my own concept of sensation. I have written something about sensation, I suppose, at two year intervals ever since 1896; and when I started out, that concept was the foundation stone of the system which I was proposing some time to build. At every stage of my own growth I gave a formal definition of the concept, and I think that any reader could tell for any particular year precisely and unequivocally what I meant by it. But I kept the concept, of set purpose, fluid from the very start; and the result is that I have carried it through a very considerable number of marked changes in meaning, every one of which followed perfectly naturally from the definition immediately preceding; and that now I am able to shelve even this concept itself for something still more fluid and still more pregnant.[34]

If you ask what Wundt meant by apperception in the fourth edition, you have no end of trouble in coming to a decision; you have to take into account all the three preceding editions as well as the criticisms that played upon the book and influenced Wundt's own thinking. If, contrariwise, you ask what I meant by sensation in the year 1904, it is a perfectly easy matter to find the nearest published article or book, and take the formal definition there given. I think that this comparison is fair, even if it is a comparison of great things with small. I use it simply to illustrate the point on which I differ from you, —the point, namely, that a fluid concept is always at any given time clear and unequivocal, whereas a concept that hardened some years back is always at the present time equivocal and ambiguous.

In the particular context of your paper on the Stimulus Error my feeling is that you have done more than define your concepts for working purposes; you have stamped them out whole and offered them as permanent currency. When one does that, my experience is that either one proceeds to shift the meaning intrinsically, as was Wundt's way, or one has definitely to revoke and take back the original pronouncement. There is no harm in revoking, of course, but it is an awkward proceeding, since the rank and file always follow you at a distance, and are therefore very likely to miss the point that you have in fact revoked.

So, you see, to my mind the maneuvering of which you speak has to be done when the concepts are hard; so long as they are fluid there is no need of any such juggling because, while they are exact working tools for the moment, they contain within them a potency of adaptation for the future. I wish that all this need not be so abstract; and I have taken the concrete cases of Wundt and myself in order to bring the point of view, if possible, more closely to you. I felt this defect in Wundt, as I had also felt it in Herbert

[34] This is Titchener's answer as to whether he shifted ground under Rahn's attack; cf. foot-note 7 and text *supra*.

Spencer, long before I had myself any notion of a logical remedy or any concepts that I could properly call my own. Frazer, in the Golden Bough, is another instance of a man who hardens his concepts from the start and therefore comes out with an utterly inadequate psychology; Tylor, on the contrary, positive as he was about his doctrine of animism, nevertheless managed to keep fluid to the very end,—with the result that animism has passed over, without any special difficulty, into the more modern ways of social anthropology.[35]

It is now ten years since Titchener died. America has taken the lead in psychology from Germany, and the line of battle that Titchener maintained has disintegrated. There are still controversies and disagreements, but the sharp separation of the psychological field into opposing camps is gone. After all, America introduced these polemics of the schools from Germany; they were not indigenous here. We are in no position to evaluate the present as against the recent past, and my relation to Titchener renders the objectivity of my appraisal of his work problematical.[36] I have printed these excerpts from his letters in the hope that I might thus help to preserve for the new generation of psychologists and the others who will come later some understanding of the intensity of faith and the magnitude of the conviction of right that Titchener put into his defense of existential mental process as the proper subject-matter of psychology. It was the insecurity of the new experimental psychology that bred in him and his contemporaries this vehemence which—with psychology's success—is not likely ever to occur again.

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[35] October 10, 1923.

[36] But cf. Boring, Edward Bradford Titchener, 1867-1927, this JOURNAL, 38, 1927, 489-506.

MATHEMATICAL ABILITY, REASONING, AND ACADEMIC STANDING

By MARGARET F. WASHBURN, Vassar College
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(Founded in 1887)

The purpose of the present study[1] was to determine the relationship of mathematical ability to reasoning and academic standing.

The *S*s, 113 in number, were undergraduate students in Vassar College. They were selected upon the basis of their ability in college mathematics and divided into two groups. One group consisted of 54 students reported by the department of mathematics as having marked mathematical ability; the other group comprised 59 students reported as decidedly low in mathematical ability.

All the *S*s were given a test in logical reasoning that was not mathematical in its nature. The test consisted of the following 20 arguments.

(1) The express train alone does not stop at this station; and as the last train did not stop, it must have been the express train.

(2) If light is not refracted near the surface of the moon, there cannot be any twilight on the moon; but if the moon has no atmosphere, light is not refracted near its surface; therefore, if the moon has no atmosphere, it can have no twilight.

(3) Every incident in the narrative is probable, hence the narrative may be believed.

(4) All civilized peoples are progressive; all uncivilized peoples are superstitious; therefore some superstitious peoples are not progressive.

(5) All who consider him diligent must approve of this student, and as you approve of him you must consider him diligent.

(6) Democracy is not a perfect form of government, for under it there are able men who do not get power, and so it allows men to get power who are not able.

(7) Whatever tends to withdraw the mind from pursuits of a low nature deserves to be promoted; classical learning does this, since it gives us a taste for intellectual enjoyments; therefore it deserves to be promoted.

(8) Aristotle must have been a man of extraordinary industry, for only such a man could have produced his works.

(9) His imbecility of character might have been inferred from his inclination to have favorites, for all weak princes have this failing.

(10) He that is of God heareth God's words: ye therefore hear them not because ye are not of God.

[1] This study was made with the coöperation of Miss Virginia Pearce and Miss Katherine Payne. In the illness of Professor Washburn, her notes were transcribed by Dr. Polyxenie Kambouropoulou.

(11) Few towns in the United Kingdom have more than three hundred thousand inhabitants: as all such towns ought to be represented in Parliament by three members, it is clear that few towns ought to have three representatives.

(12) Some mineral compounds are not decomposed by heat; all organic substances are decomposed by heat; therefore no organic substances are mineral compounds.

(13) No soldiers should be brought into the field who are not well qualified to perform their part; only veterans are well qualified to perform their part; therefore only veterans should be brought into the field.

(14) If light consisted of material particles, it would possess momentum; it cannot consist of material particles, for it does not possess momentum.

(15) No war is long popular, for every war increases taxation, and the popularity of anything that touches the pockets is short-lived.

(16) If the students of this college do not take their work seriously, the college will gradually lose prestige; but the students are taking their work seriously, hence the college will not lose prestige.

(17 It must be borne in mind that some spiritistic mediums have never been proved to be frauds; also that some mediums never ask money for their services. Thus we may say that investigators have demonstrated fraud only in the case of those individuals who have wished to make financial gain from their supposed powers.

(18) No event in whose presence dispersion of clouds fails to appear can be the cause of dispersion of clouds. A rising moon is an event in whose presence dispersion of clouds may fail to appear. Hence a rising moon is not the cause of the dispersion of clouds.

(19) The following criticism was made on Mach's announcement of his discovery of the sense of equilibrium: If a sixth sense existed, it could not have failed to have been discovered thousands of years ago. Mach answered: "Indeed? There was a time then when only seven planets could have existed!"

(20) The belief of the natives of Borneo that only he is brave who has taken the head of an enemy is a false belief. Hence, since none of your enemies has lost his head through your endeavors, you are a brave man.

Every one of these arguments was judged as logically sound or false by every *S*. The answers with the time required to make them, measured with a stop watch, were recorded without comment by *E*.

Besides the *S*s' records in mathematics and in this test of logical reasoning, the *S*s' general academic standing was available. Comparisons of these various records reveal the following results.

Results

The relation of mathematical ability to reasoning. The average number of errors in reasoning made by the group of 54 *S*s good in mathematics was 4.74 errors. The average number of errors made by the group of 59 *S*s poor in mathematics was 6.30 errors.

A group with good reasoning ability was selected by taking the 38 *S*s (34% of the whole group) who had made 4 or fewer errors in reasoning. That left 75 *S*s (66% of the whole group) who had made 5 or more errors in reasoning and were considered poor in reasoning.

In the group of 54 *Ss* good in mathematics there were 28 (52%) who made 4 or fewer errors in reasoning, and 26 (48%) who made 5 or more errors in reasoning.

In the group of 59 *Ss* poor in mathematics there were 10 (17%) who made 4 or fewer errors in reasoning, and 49 (83%) who made 5 or more errors in reasoning.

In the group of 38 *Ss* selected as good in reasoning, 28 (73%) were good in mathematics and 10 (27%) were poor in mathematics.

In the group of 75 *Ss* considered poor in reasoning, 26 (35%) were good in mathematics and 49 (65%) were poor in mathematics.

Conclusions. (1) Mathematical ability and reasoning ability as represented by the above test are related.

(2) Good mathematical ability does not guarantee good reasoning as much as good reasoning guarantees good mathematical ability. Whereas in the group of those good in mathematics the proportion of good reasoners to poor is 1.08, or 28:26, in the group of good reasoners the proportion of those good in mathematics to poor is 2.8, or 28:10.

(3) Poor mathematical ability excludes good reasoning more than poor reasoning excludes good mathematical ability. Whereas in the group of those poor in mathematics the proportion of good to poor in reasoning is 0.2, or 10:49, in the group of those poor in reasoning the proportion of good to poor in mathematics is 0.53, or 26:49.

(4) According to the standard of reasoning ability set by this test, it is harder to be good in reasoning than to be good in mathematics. It is more probable that one who is good in reasoning will be good in mathematics than that one who is good in mathematics will be good in reasoning. Also it is more possible for one who is poor in reasoning to be good in mathematics than for one who is poor in mathematics to be good in reasoning.

These results suggest the following explanation. Mathematical reasoning is abstract; it deals with quantity, which is a homogeneous concept; while the relations with which non-mathematical reasoning deals are complicated. Also the process of reasoning with non-mathematical, non-quantitative relations is more likely to be interfered with by preconceived ideas, extraneous to the reasoning itself, as to the rightness or wrongness of the conclusion than is the process of reasoning in mathematics.

The relation of high academic standing to reasoning ability and to mathematical ability. Of the 113 *Ss* to whom the reasoning test was given,

34 were selected as being of decidedly high academic standing and 17 as being of decidedly low academic standing.[2]

The average number of errors in reasoning made by the 34 *Ss* of high academic standing was 4.74 errors. The average number of errors in reasoning made by the 17 *Ss* of low academic standing was 6.7 errors.

In the group of 34 *Ss* of high academic standing there were 17 (50%) who made 4 or fewer errors in reasoning, and 17 who made 5 or more errors in reasoning.

In the group of 17 *Ss* of low academic standing there was one (6%) who made only 2 errors in reasoning, and 16 (94%) who made 5 or more errors in reasoning.

In the group of 34 *Ss* of high academic standing, 24 were good in mathematics as well as in other subjects, and 10 were poor in mathematics but good in other subjects. The average number of errors in reasoning made by the 24 of high academic standing and good in mathematics was 4.7 errors. The average number of errors in reasoning made by the 10 of high academic standing but poor in mathematics was 4.9 errors. In the group of 24 of high academic standing and good in mathematics, 13 (54%) made 4 or fewer errors in reasoning, and 11 (46%) made 5 or more errors in reasoning. In the group of 10 of high academic standing but poor in mathematics, 4 (40%) made 4 or fewer errors in reasoning and 6 (60%) made 5 or more errors in reasoning.

In the group of 17 *Ss* of low academic standing, 3 (18%) were good in mathematics but poor in other subjects, and 14 (82%) were poor in mathematics as well as in other subjects. The average number of errors in reasoning made by the 3 of low academic standing but good in mathematics was 8 errors. The average number of errors made by the 14 of low academic standing and poor in mathematics was 6.4 errors. In the group of 3 of low academic standing and good in mathematics, all 3 (100%) made more than 5 errors in reasoning. In the group of 14 of low academic standing and poor in mathematics, one (7%) made 4 or fewer errors in reasoning, and 13 (93%) made 5 or more errors in reasoning.

Of the 18 *Ss* in both the high and low academic groups who made 4 or fewer errors in reasoning, 17 (94%) were of high academic standing, and one (6%) was of low academic standing. Of the same 18,

[2] It was difficult to find a large group of very low academic standing, as these are not allowed to continue in college.

13 (72%) were good in mathematics and in other subjects, 4 (22%) were of high academic standing but poor in mathematics, and one (6%) was of low academic standing and poor in mathematics.

Conclusions. (1) Intellectual ability, as represented by high academic standing, and reasoning ability, as represented by the above test, are related.

(2) High academic standing does not necessarily guarantee good reasoning ability, but good reasoning ability is an excellent guarantee of high academic standing.

(3) Poor reasoning ability does not exclude high academic standing, but low academic standing excludes good reasoning ability (with one exception in this test).

(4) Good reasoning ability guarantees high academic standing more than it guarantees high academic standing plus good mathematical ability. Whereas, with one exception, all the good reasoners are good students, 72%, or 13 out of 18, are good in mathematics in addition.

(5) Where academic standing is high, reasoning ability is only a little higher in the group that is good in mathematics than in the group that is good in other subjects only. The ability in reasoning with mathematical, quantitative relations does not add much to the ability to reason with non-quantitative relations.

(6) Where academic standing is low, good reasoning ability is absent (except in one case). Mathematical ability, however, may exist (3 out of 17) without good reasoning ability and with low academic standing. The ability to reason with quantitative relations seems to be a specific ability in these 3 cases.

Time. *Ss* of low academic standing took less time with the test than those of high academic standing, and also had the greater number of errors in reasoning. Within the group of high academic standing, however, time and errors were not related. The inference is, therefore, suggested that the better students gave the test more serious consideration.

THE AMERICAN JOURNAL OF PSYCHOLOGY: 1887-1937

By KARL M. DALLENBACH, Cornell University
Editor, *The American Journal of Psychology*
(Founded in 1887)

THE AMERICAN JOURNAL OF PSYCHOLOGY, whose Golden Jubilee is signalized with this Volume, was founded by G. Stanley Hall in November, 1887. It was the first periodical in the English language to be devoted entirely to psychology, and it is today the oldest psychological journal in existence. Wundt's *Philosophische Studien*, which antedates the JOURNAL by six years, lapsed in 1903; it was, moreover, essentially an organ of the Leipzig laboratory, whereas the JOURNAL was from its inception open to all workers in psychology.

HALL'S JOURNAL

The story of the JOURNAL's founding, as related by Hall in several places, goes back to *Mind*, which was established in 1876. Croom Robertson, Hall says, "told me the interesting story of its inception in a way that filled me with emulation."[1] *Mind*, founded by Alexander Bain and edited until 1892 by Croom Robertson, professor of philosophy and logic at University College, London, was not, nor was it ever to be, the medium of the new psychology which was then becoming articulate. It was at that time, as it is now, a liberal philosophical journal, welcoming articles of psychological import. "Both its spirit and its field," Hall writes, "were very different from the journal I intended, which I wanted to have, first of all, an exponent of experimental laboratory psychology."[2]

The opportunity of emulating Bain and Robertson and of founding a journal after his own desire came to Hall one Sunday afternoon in 1887 during an unexpected visit "from a wealthy stranger from Philadelphia, J. Pearsall Smith, who had heard of a new department of psychology and who suggested that I start a journal, handing me on the spot a check for five hundred dollars to that end, with the intimation that

[1] G. S. Hall, The American Journal of Psychology, this JOURNAL, 32, 1921, 1-3.
[2] *Ibid.*, 1.

more would follow. . . This was the origin of The American Journal of Psychology."[3]

Hall's plans and hopes, which differed radically from those espoused by *Mind*, are clearly set forth in an editorial note in the first number,[4] where he declared that "the object of the Journal is to record the psychological work of a scientific, as distinct from a speculative character," and he contracted with his public to "bear in mind . . . teachers of psychology in higher institutions of learning; biologists and physiologists; anthropologists who are interested in primitive manifestations of psychological laws; physicians who give special attention to mental and nervous diseases; all others interested . . . in applying more exact methods to the study of the problems of human feelings, will and thought." He promised, furthermore, that "the advancement of the science will be constantly kept in view, and the journal will be a record of the progress of investigations." An ambitious program that was equalled only by his enthusiasm!

To fulfill this program, Hall divided the JOURNAL into three department, as follows: "I. *Original contributions of a scientific character.* These will consist partly of experimental investigations on the functions of the senses and brain, physiological time, psycho-physic law, images and their association, volition, innervation, etc.; and partly of inductive studies of instinct in animals, psychogenesis in children, and the large fields of morbid and anthropological psychology, not excluding hypnotism, methods of research which will receive special attention; and lastly, the finer anatomy of the sense-organs and the central nervous system, including the latest technical methods, and embryological, comparative and experimental studies of both neurological structure and function. II. *Digests and reviews.* An attempt will be made in each number to give a conspectus of the more important current psychological literature, and to review significant books, bad as well as good. III. *Notes, news, brief mentions,* etc."[5]

Hall planned to publish quarterly and 600 pages annually. He did both the first year,[6] and Sanford, who edited the second volume[7] while

[3] Hall, A reminiscence, this JOURNAL, 28, 1917, 299. See also his *Life and Confessions of a Psychologist*, 1923, 227.

[4] Hall, Editorial note, this JOURNAL, 1, 1887, 3-4.

[5] *Idem.*

[6] The first number contained 209 pages, the first volume 759 pages. It was 45 years before these lengths were surpassed: Vol. 44, 1932, was 866 pages in length; No. 1 was 220 pages, and No. 4 was 252 pages.

[7] Sanford edited the second volume while Hall was aboard, though it carries Hall's name as editor and makes no mention of Sanford's editorship. In his *Life and Confessions*

Hall was abroad studying foreign institutions preparatory to the opening of Clark University, also succeeded in doing both the second year.[8] Then the supply of publishable material, which had never been great, became scant. The Hopkins laboratory that Hall founded had been allowed to lapse with his departure, and the few laboratories then existing in America[9] could not supply sufficient copy to fill the requisite 600 pages. Rather than reduce the size, Hall broadened the scope of the JOURNAL to include non-experimental articles in the fields of philosophy, logic, esthetics, and religious, educational, pathological, and comparative psychology, and when even this measure proved insufficient, he filled the pages of the JOURNAL with summaries and book reviews written largely by himself.[10] This took time of which he, then presiding over a newly founded university, had very little, and Volumes 3 and 4 dragged out over three years.[11] Volume 5 (1892-1893), however, appeared within a span of a year; by then the psychological laboratories founded during the late 80's and early 90's were beginning to function, and increasing amounts of acceptable copy were available.

More grievous even than the editorial troubles were the financial. Subscribers were few in number, "only a little over a hundred" at the beginning of the second volume,[12] and the JOURNAL's only patron, who was interested in psychical research and who thought that Hall would favor this cult,[13] was estranged by the appearance, in the first number, of a severely critical 19-page review of the 'experiments' published in the first 5 volumes of the *Proceedings of the English Society for Psychical Research*.[14] Hall had, in consequence, to draw deeply upon his own re-

(p. 263) Hall writes, "I made no final engagements for the [Clark] staff [before leaving on the European tour] save in one or two important instances, notably that of Dr. Sanford, who was to edit my Journal for a year." Again, in his valedictory note in this JOURNAL (32, 1931, 2), he says that through "the generosity of Jonas G. Clark . . . [Dr. Sanford] was employed to edit the *Journal* during 1888-1889, when I was abroad." Apparently, Sanford devoted all of his time to the editing of that volume.

[8] The second volume was 685 pages in length. It was 15 years before this volume was to be surpassed in size. (Vol. 14, 1903, containing a large double number commemorating the 25th year of Hall's doctorate, was 698 pages.)

[9] Cf. C. A. Ruckmick, The history and status of psychology in the United States, this JOURNAL, 23, 1912, 520; also C. R. Garvey, List of American psychological laboratories, *Psychol. Bull.*, 26, 1929, 652-660.

[10] Volume 3, which gave Hall the greatest editorial difficulty, allotted more space to reviews (57%) than to original contributions. The second number had, for example, 111 pages of reviews and 33 pages in original contributions.

[11] The assistance of E. W. Scripture is acknowledged on the covers of numbers 3 and 4 of Volume 4.

[12] *Op. cit.*, this JOURNAL, 28, 1917, 299.

[13] *Op. cit.*, *ibid.*, 32, 1921, 2.

[14] *Op. cit.*, *ibid.*, 1, 1887, 128-146.

sources. "At the end of a few years," he tells us, "I had sunk over eight thousand dollars of my own hard earnings in the Journal."[15]

Hall persevered and, with Volume 5, both problems seemed to be well upon the way to a happy solution. Then competition for contributors and subscribers appeared in the form of *The Psychological Review* and its collaterals, the *Psychological Monographs* and the *Psychological Index*, founded by J. Mark Baldwin and J. McKeen Cattell—the first in 1894 and the latter two in 1895.

It has been said that these periodicals were founded to "offset" the dominance of the JOURNAL, in response to a general complaint that the JOURNAL "was made to serve primarily the interests of Hall and his associates in other universities."[16] It is difficult now to say whether there is any foundation of fact to this charge. It may have originated as a *post hoc* explanation for the establishment of the Psychological Review publications. Certain it is that neither Cattell nor Baldwin could have had any cause for complaint. The record shows that they published frequently and were adequately reviewed in the JOURNAL,[17] and that they published original articles in Volume 5, the volume immediately preceding the appearance of their journals. Cattell, furthermore, who, as one of Hall's former students, would have been numbered among the "associates in other universities" and so, exempted from the alleged discrimination, could hardly have led a revolt against an oppression that he did not feel. There was also, as far as records go, no animosity on Hall's part toward the new venture. Sanford,[18] Hall's colleague at Clark, who was identified with the JOURNAL from its beginning, and a number of Hall's former students contributed to the first number of *The Psychological Review* which they certainly would not have done had there been any hostility between the two groups.

It is probable that Cattell and Baldwin were motivated by a desire to serve. Publication was becoming congested in psychology; the demand for space exceeded the supply, as it has many times since, and with similar results: new journals. Hall tried to meet the demand by reducing the amount of space given to reviews from 57% in Volume 3 to 13% in Volume 5,[19] but this did not suffice. The vigor and growth

[15] *Op. cit.*, this JOURNAL, 28, 1917, 299.

[16] E. G. Boring, *A History of Experimental Psychology*, 1929, 616.

[17] Baldwin published in Vols. 3, 1890, 285; 3, 1891, 593; 5, 1892, 272; 5, 1893, 420; 5, 1893, 549. Cattell in Vols. 3, 1890, 281; 4, 1892, 460; 4, 1892, 539; 5, 1893, 285. Reviews: Baldwin, 2, 1889, 669; 4, 1892, 468. Cattell, 1, 1888, 708 (3 items); 2, 1888, 166; 2, 1889, 487; 3, 1890, 383; 5, 1893, 549.

[18] E. C. Sanford, *Psychol. Rev.*, 1, 1894, 97 ff.

[19] The first number of Volume 6, published in October 1893 before the competition of the

of the 'new' psychology called for more 'space' than a single journal could supply.

The new journals trebled at once the amount of available space, and again, as in the early years of the JOURNAL, the demand was exceeded. Reviews had to be used as fillers, and they bulked large both in *The Psychological Review* (36%) and in the two numbers of the JOURNAL issued in 1894 (22%). The advantage in the competition for copy was with the new journals. Hall stood alone, whereas Cattell and Baldwin had the support of an international coöperating board of ten illustrious psychologists. Position on this board was not honorary; there is proof of genuine coöperation in the early issues of the Psychological Review publications.

Twelve men collaborating on a common task made it easy as compared to Hall's. Rather than to cut the subsequent issues to the supply of material in hand, Hall delayed publication until full-size numbers could be issued. Volume 6 covered two years. It was begun in October 1893 and completed in January 1895. Then nine months elapsed, until October 1895, before another number appeared. During the interim, Hall reorganized the JOURNAL. In the first issue following this lapse, after reviewing the history and accomplishments of his periodical, Hall announced certain changes in policy.[20]

In his historical review, Hall did not differentiate between himself and his journal and, in consequence, claimed for it everything that he and it had accomplished in psychology. Some of his statements—for example, that the JOURNAL represented the department of psychology at Hopkins and at Clark, whose students founded the laboratories "at Harvard, Yale, Philadelphia, Columbia, Toronto, Wisconsin, and many other institutions of learning;" and that it "invited and presided over the establishment of the American Psychological Association"[21]—seem, particularly when read alone or taken from their context, excessive or inaccurate. The only statement to be challenged, however, was the one concerning the founding of the various laboratories. For this Hall was sharply called to account by James, Ladd, Baldwin, and

new journals was felt, contains but 6 pages of reviews, a further indication of Hall's attempt to serve his confrères by making more space available for original contributions.

[20] Hall, Editorial, this JOURNAL, 7, 1895, 3-8.

[21] As a matter of historical fact, the group that organized the American Psychological Association was called together by Hall and met in his study on July 8, 1892. Cf. S. W. Fernberger, The American Psychological Association: A historical summary, 1892-1930, *Psychol. Bull.*, 29, 1932, 2; also Hall, *Life and Confessions*, 1923, 8.

Cattell.[22] Open hostility was avoided when Hall took cognizance of these criticisms, explained and corrected his statements, and concluded with the pious hope "that in this transition period the psychological atmosphere will not become too tense for a spirit of hearty coöperation, or too lax for healthful or virile competition."[23] Remembrance of the feelings engendered in this controversy, displaced backward in time, is probably responsible for the legend, mentioned above, that the Psychological Review publications were founded in protest to "offset" the JOURNAL.

Of the changes in policy announced, the most important was the division of Hall's editorial responsibility with Sanford and Titchener[24] and an international board of coöperating editors consisting of F. Angell of Stanford University, H. Beaunis of the Universities of Nancy and Paris, J. Delboeuf of the University of Liège, V. Henri of Leipzig (later of Paris), C. H. Hodge of Clark University, A. Kirschmann of the University of Toronto, O. Külpe of the University of Würzburg, A. Waller of London, and H. K. Wolfe of the University of Nebraska. Since then the JOURNAL has been a coöperative affair.

Among the other changes announced were "a new department of notes;" revision of the review department to insure that it will not be "limited to what publishers choose to send;" "exclusion of all advertisements whatever, save occasional friendly exchanges with other scientific journals in cognate lines;"[25] extension of the JOURNAL to include the "*Archiv* function, not yet represented by any serial publication in this field of English;" and the promise to publish manuscripts "in the order of receipt." The policy was reaffirmed of keeping the JOURNAL general in scope and of including, in addition to experimental investigations, scientific "studies in abnormal psychology, including the insane, criminals, idiotic, blind, deaf, or other defectives or degenerates;" "the anthropology of myth, custom, religious belief, symbols, etc., among savage and ethnic stocks;" "genetic psychology and exact and careful child-study by scientific observers;" "animal psychology;" "neurological researches;" and "the psychology of philosophy, ethics,

[22] William James, Experimental psychology in America, *Science*, 2, 1895, 626; G. T. Ladd, *idem*, 626 f.; J. M. Baldwin, *idem*, 627; J. McK. Cattell, *idem*, 672 f.

[23] Hall, *idem*, 734 f.

[24] They are recorded in the first number of Volume 7 as assistant editors. This, Hall writes (*Science*, 2, 1895, 735), "Dr. Sanford desires me to state, was his regrettable mistake, and will be corrected according to the original announcement, in the next number."

[25] As the JOURNAL had never carried any advertisements other than a few friendly exchanges with other scientific journals, this was a gesture in condemnation of the commercial advertisements in *The Psychological Review*.

aesthetics, theology, etc." Yet the field of the JOURNAL did "not include the history of philosophy, ethics, or pedagogy, . . . epistemology, metaphysics, sociology, theology unless treated (psychologically), nor . . . long discussions concerning matters on which conclusions are impossible."

From the reorganization of 1895, due more perhaps to the tremendous strides of psychology in America than to the new alliances and policies, Hall's editorial troubles were over and his periodical appeared regularly four times a year. From 1895 to 1901 (Vols. 7-12) the volumes followed the academic year, the first number appearing in October and the last in July. To simplify bibliographical reference, the first number of Volume 13 was delayed until January 1902 and thenceforth the volumes have coincided with the calendar year.

The personnel of the first editorial board changed but little during Hall's management. Sanford, Titchener, Angell, Beaunis, and Waller continued until Hall's retirement. J. W. Baird of Clark University was co-editor from 1912 until his death in 1919. Vacancies due to retirement and death were filled by G. W. Störring of the University of Leipzig in 1896, A. F. Chamberlain of Clark University and W. B. Pillsbury of the University of Michigan in 1897, M. Bentley of Cornell University and M. F. Washburn of Vassar College in 1903. In all it was a small group that assisted Hall in guiding and shaping the JOURNAL for twenty-five years.

Until the War and the post-War years all the volumes of the JOURNAL were approximately 600 pages in length. The 1918 volume and the two following, the last three to be published under Hall's editorship, were considerably smaller[26]—as were most scientific magazines during that period. Lack of material—the sterile years of the War were making themselves felt—was not the only reason; copy in the form of summaries and reviews could have been obtained. A more compelling reason was finance—also a product of the War. Most of the foreign subscriptions, of which there had been many, and a considerable number of the domestic subscriptions were cancelled. The financial loss was aggravated further by a marked (20%) increase in the cost of printing.

As we have seen, the JOURNAL had once before been faced with similar conditions and Hall had personally met them. Now in the evening of of his life, when his energy and enthusiasm were at low ebb, he did nothing to alter or to improve conditions. He followed the path of

[26] The decrease in size was about a third. The volumes for 1918, 1919, and 1920 were respectively 457, 433, and 416 pages in length.

least resistance and when, in June 1920, he laid aside his duties as president of Clark University at 74 years of age, he was also ready to dispose of his periodical.

Titchener's Journal

In December 1920, Hall sold the Journal to Dallenbach of Cornell University. The transfer was made with the understanding that the Journal would be edited by Titchener, who, Hall wrote in his valedictory note, "is my natural successor and heir . . . and I am confident that under his guidance the Journal will enter upon a larger career of service than it has hitherto known."[27] Titchener was given a free hand; he was made sole editor to do and to make what he would of the Journal.

In addition to Dallenbach, who handled all the business affairs and saw the numbers through the press, Titchener, selected as coöperating editors: Hall, Sanford, and E. G. Boring of Clark University, H. P. Weld of Cornell University, Madison Bentley of the University of Illinois, W. B. Pillsbury of the University of Michigan, Frank Angell of Stanford University, and M. F. Washburn of Vassar College. To this number he added in 1925 (after the death of Hall and of Sanford, and Boring's transfer to Harvard) E. S. Robinson of the University of Chicago, J. P. Nafe of Clark University, L. B. Hoisington of Cornell University, and C. A. Ruckmick of the University of Iowa. Position on the board was, however, chiefly honorary, as the editorial tasks were carried entirely by Titchener and questions of policy, except as they touched upon financial matters, were decided solely by him. Even routine matters, such as the assignment of books to review, were personally attended to by him.

Titchener's immediate task was, as he stated in his first number in a brief response to Hall's valedictory note, "to make the future Journal worthy of the past."[28] To this end the Journal was immediately brought up to its pre-War size; the first number was 160 pages in length as against 100 pages the year before. To meet expenses, the annual subscription price, which had remained at $5 through all the years of increasing printing costs since 1888, was raised to $6.50 (the price at which it has since remained) and the pages were opened to such commercial advertising as was consistent with the function of a scientific journal.

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[27] Hall, *op. cit.*, this Journal, 32, 1921, 3.
[28] E. B. Titchener, *ibid.*, 4.

The catholicity of Hall's editorial point of view was maintained by Titchener. He divided the JOURNAL into five departments: major articles, discussions and minor communications, reviews of books, book notes, and miscellaneous notes; and on the editorial page (the inside front cover) he ran the following: "While the Journal was founded in the interest of experimental psychology, and has always been largely devoted to that interest, its pages are open to workers in all other fields of scientific psychology." This pronouncement of policy was adhered to throughout his editorship and down to the present day. The JOURNAL soon regained, under Titchener, its former position among the scientific periodicals of the world.

THE PRESENT JOURNAL

Titchener edited five volumes, Volumes 32-36 (1921-1925). He retired in October 1925. The editorial management then passed into the hands of Washburn, Bentley, Boring, and Dallenbach, all of whom had previously served the JOURNAL as coöperating editors, the first two since 1903 and the last two since 1921. All were former students of Titchener and all had completed the doctorate with him at Cornell University. The order in which the names of the editors appeared on the title and cover pages was varied during the course of the succeeding years. In 1936 (Vol. 48) Dallenbach's name was, at the firm insistence of his colleagues, placed first.[29]

The new editors invited a representative group of American psychologists to assist them. The following accepted: Raymond Dodge, Yale University; S. W. Fernberger, University of Pennsylvania; J. P. Nafe, Clark University; R. M. Ogden, Cornell University; Joseph Peterson, Peabody College; W. B. Pillsbury, University of Michigan; A. T. Poffenberger, Columbia University; E. S. Robinson, University of Chicago; C. A. Ruckmick, University of Iowa; and G. M. Stratton, University of California. Except for the death of Joseph Peterson in 1935[30] and of E. S. Robinson in 1937,[31] this board has remained unchanged to the present.

No changes in policy were immediately made. In announcing the retirement of Titchener and their assumption of the management, the

[29] M. F. Washburn. M. Bentley, and E. G. Boring, Editorial note, this JOURNAL, 48, 1936, 177.
[30] E. S. Robinson and F. R. Robinson, Joseph Peterson: 1878-1935, *ibid.*, 48, 1936, 175-177.
[31] H. A. Carr, Edward Stevens Robinson: 1893-1937, *ibid.*, 49, 1937, 488-489.

new editors pledged themselves to "try to maintain it at the high standard set by Professor Titchener."[32] Such changes as have since been made have come slowly and only after mature consideration by the entire Board, which met annually for a number of years to consider such questions of policy as any of its fourteen members wished to bring up. Among the proposals adopted by the Board during the course of years are the following.

(1) *Stylebook.* Hall was little interested in questions of style. He was more concerned with substance than with form and in consequence there is little uniformity among his volumes. The editorial style varied markedly in successive numbers and even among the articles in any given number. Titchener paid more attention to it but he did not work with a stylebook in mind and consequently he too fell short of uniformity within any given number or volume. Under four editors, the need for a stylebook and of uniform editing was keenly felt. Unless the work was done with a standard in mind the JOURNAL would be an editorial hodgepodge. The Board thereupon adopted the stylebook of the U. S. Government Printing Office, modifying it between volumes as conditions demanded. THE AMERICAN JOURNAL now spells in the American way—except when it publishes an article by an English confrère who is permitted, in courtesy, to spell after his own fashion. Tables are numbered in Roman; numbers and illustrative cuts, designated "Figures," in arabic. Explanatory captions are run above tables and legends below figures. Sectional headings when centered appear in capitals; when placed as marginal stubs, in italics.

The JOURNAL, furthermore, dispenses with terminal bibliographies and prints all references in the form of consecutively numbered footnotes. The board decided upon this practice in order to save space, for which there was a great demand, and for the convenience of its readers—since it is easier to cast one's eyes to the bottom of the page when a citation is made than to turn to the end of the article. Other considerations were the avoidance of duplication, as most of the terminal bibliographies merely bring together items referred to in the footnotes, and of padding, *i.e.* the inclusion of items not referred to in the text—listed perhaps to impress the unsophisticated with the writer's erudition. (Surely, if a reference is not of enough importance to be cited in the text it is not of great significance to the text!) Oversized tables and graphs, which had to be printed separately and folded and inserted, were also eliminated. These inserts, as experience has shown, are frequently lost or cut in binding and torn out and destroyed in reading; they are, also, awkward and inconvenient to use.

(2) *Apparatus department.* In 1927 a new department, devoted to descriptions of psychological apparatus, was added.[33] This did not mark a change in policy, as the pages of the JOURNAL had always been open to articles of this kind. It did, however, add to the convenience of the readers, as articles on apparatus had heretofore been scattered among the different departments—sometimes appearing among the major articles, sometimes among the minor communications, and at other times among the Notes. The new department met with instant success, and it has, in consequence of the support it has received from readers and contributors, been represented in every subsequent number. Other psychological periodicals have since added similar departments. The number of departments in the

[32] M. F. Washburn, K. M. Dallenbach, Madison Bentley, and E. G. Boring, An announcement, this JOURNAL, 37, 1926, 1.

[33] K. M. D., A new department: Apparatus, *ibid.*, 38, 1927, 316.

JOURNAL still remained at five: Articles, Discussions and Minor Communications, Apparatus, Book Reviews, and Miscellaneous Notes. Titchener's department of Book Notes was abandoned.

(3) *Notes and discussions.* The next year, in 1928, a department of Notes and Discussions was added, combining part of Discussions and Minor Communications with Miscellaneous Notes. In this new department the editors agree to "print all discussions of articles, replies to them, criticism of them, or controversy about them, . . . also long informatory notes and discussions, and all material of interest to psychologists that does not properly belong elsewhere."[34] Because of the pressure for space, this department from the outset was printed in 8-point type.

The departments, still five in number, were now as follows: Articles; Minor Communications; Apparatus; Book Reviews; and Notes and Discussions. In 1930 the positions of Book Reviews and Notes and Discussions were interchanged to simplify the task of make-up, it being considerably easier to take up the slack of a number in the Book Reviews than in any other section.

(4) *Dating of manuscripts.* In 1926, at the annual meeting of the Board held during the Christmas holidays, it was decided to follow the procedure of many psychological periodicals in Europe in publishing with every article the date of its acceptance. Because the January 1927 number was on the press at the time this decision was made and the Board did not wish to initiate a change in style in the midst of a volume, the policy was not made effective until January 1928.[35] In the meantime, most of the psychological periodicals in America adopted the same policy.

This policy was adopted because of the long lag in the publication which at that time was over a year behind acceptance. Something had, therefore, to be done in order to protect, as well as to establish, the priority-claims of the JOURNAL's contributors. Publishing the date that manuscripts were accepted seemed a happy solution, but it raised some problems, however, that had not previously existed, *e.g.* balanced make-up and place-crediting.

Though committed to the general policy of publishing articles in order of the receipt, the editors had long been accustomed to exercise judgment in making up a number. They attempted in every issue to achieve a proper balance between long and short articles. This meant that publication could not always follow strictly in the order of receipt. Sometimes long articles had to be delayed and short articles advanced, and at other times the opposite had to be done. As long as the dates of receipts were known only to the editors, the make-up occasioned no embarrassment, but as soon as a policy of publishing those dates was decided upon the problem of make-up came to the fore. If publication followed absolutely the order of receipt some numbers would be made up entirely of long articles and others entirely of short. Balance would result only by chance. In order to avoid a haphazard make-up and at the same time the embarrassment that would result from running articles out of turn, the Board when announcing the policy regarding the dating of manuscripts, gave notice that articles would be classed as long and short, and that within each class publication would follow order of receipt insofar as that was consistent with a balanced number. Notice was also given that articles accepted for the Apparatus and the Notes and Discussions departments, since they were short and usually timely, would be run at the earliest possible date.[36]

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[34] E. G. B., Editorial departments, this JOURNAL, 40, 1928, 171.
[35] K. M. D., The dating and publication of manuscripts, *ibid.*, 40, 1928, 172.
[36] *Ibid.*

Publishing date of acceptance also brought with it the necessity of formulating a policy regarding place-crediting. Up to this time the author's desires were followed. If he changed his university or institutional affiliation between the time of a manuscript's acceptance and publication he could claim either the old or the new affiliation, as he preferred. When the articles were dated, the Board decided that the place-line should accord with the date of acceptance, and so insure the JOURNAL's accuracy as a source of historical reference. As the lag in publication became longer—it reached 21 months in 1934[37]—the place-line became more and more inadequate as an address or record of the institutional connection of the author at the time of publication. Since these two latter advantages were thought to outweigh the former, the procedure of place-crediting, after being followed for nine years (Vols. 40-48 inclusive), was changed in 1937.[38] At present, therefore, the JOURNAL gives in the place-line the author's institutional affiliation at the date of publication. A differing affiliation at the date of acceptance is recorded in the first footnote of an article.

(5) *Book reviews.* In the early years of the JOURNAL, the review department was one of the strongest, but during Hall's later years and under Titchener it was neglected.[39] It was the weakest department when the present Board took control, and it was the first that they sought to strengthen. Though more books were reviewed in their first two volumes than in the preceding five (121 against 110), the review department was still, after two years, the weakest of the JOURNAL's offerings. The difficulty was thought to be one of divided responsibility. The department was, therefore, placed in 1928 in the hands of Joseph Peterson,[40] a member of the coöperating board who had greatly assisted in the work of the department during the two preceding years. He assumed the editorship of the review department in the April 1928 number and continued it until his death in September, 1935. Given a free hand to make what he could of the department, he organized and systematized the work, gathered together a willing and an excellent group of assistants, and soon doubled and trebled the number of reviews that were published annually. Constructive and critical reviews were emphasized as over against mere abstracts. Under Peterson the department soon became one of the strongest of the JOURNAL, and the most extensive in American psychology.[41] His place has not been filled since his death—his like is not easy to find—and the department is at present moving along under the impetus that he gave it.

(6) *Type-size and font.* From its establishment, the JOURNAL had been printed solid without leading. Original articles were set in large type (10-point) or in mixed type-sizes (10- and 8-point—as historical discussions, descriptions of apparatus and experimental procedure were usually set in the smaller type) and the footnotes and all the other departments were set in small type (8-point). The first change initiated by the new Board was to differentiate footnotes from text by running them in a type-size smaller than that used in the text; when preceded by 10-point type they were set in 8-point, and by 8-point, in 6-point. Because the compositor could not always determine which size would be required under this rule, many footnotes had at considerable expense to be reset. The rule was changed, therefore, after one volume (Vol. 37, 1926) and the footnotes were uniformly set in 6-point type. This style obtained for two years.

[37] E. G. Boring, The lag in publication in journals of psychology, this JOURNAL, 49, 1937, 138.

[38] K. M. D., Editorial note: The lag in publication, *ibid.*, 49, 1937, 139.

[39] During Titchener's five years, only 110 books were reviewed—a number exceeded now in a single year.

[40] K. M. D., The book review department, this JOURNAL, 40, 1928, 352.

[41] Cf. R. S. Schultz and Helen Pallister, Book reviews in psychological periodicals, *ibid.*, 46, 1934, 508-516.

The page thus set consumed copy. That virtue, because of the pressure for space, offset for a time the criticism that the page had become difficult to read. The criticism, however, would not down, and finally as a trial to test the Board's differences of opinion, the Washburn Commemorative Volume (Vol. 39, 1927) was printed with 2 points of lead[42] (*i.e.* with 8- and 10-point type on 10- and 12-point matrices respectively) and with footnotes in 8-point solid. The comments received upon the style of that volume were so favorable and so wide-spread that the Board adopted it in 1929 for the regular issues.[43]

To compensate in part for the 18% loss in space entailed by leading, the page was at the same time increased slightly in size: in width from 23 to 25 picas, and in length from 40 to 42 picas—a gain over the old page of 14%. Increase in legibility was thus achieved at a decrease of only about 4% in amount of material offered.

In 1933 (Vol. 45), monotype was abandoned for linotype, and the font and paper-stock used in printing were changed. To compensate for the loss in number of words per page in linotype printing, the Old Style type-face was replaced by Garamont, a condensed yet legible type.[44] The new paper-stock, English finish in place of laid eggshell, reduced the bulk of the JOURNAL volume (now running well over 700 pages in length) by about a third, and also permitted free use of half-tone illustrations previously printed on glazed inserts.

(7) *Prior publication.* In 1928, in spite of rigorous selection of papers, the lag in publication became serious, manuscripts being printed about a year after their acceptance.[45] As a means of avoiding this long delay, the editors considered the publishing of two volumes a year, the establishment of a monograph series, and a plan of prior publication which had been used successfully by several journals in other sciences. The first two proposals were discarded because they would have added to the financial burden of the subscribers. Since the third would not do this, it was tentatively agreed upon and announced in January, 1930.[46] Under this plan, contributors could secure early publication by assuming part of the cost of printing their articles. In effect this increased the size of the JOURNAL, as articles published under this plan were added to the normal issue of 156 pages—624 pages per year.

The plan was favorably received by the JOURNAL's contributors and after a year's trial it was permanently adopted. Every number that has since been published has contained papers printed under that plan. The annual volumes have in consequence run from 75 to 242 pages long, and without extra cost to subscribers. Other psychological journals soon adopted a similar plan, so that the total gain to psychology has been considerable. Indeed, the cumulative journals,[47] which have since been established, represent a further development of the same idea.

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[42] The amount of leading found by D. G. Paterson and M. A. Tinker (Studies of typographical factors influencing speed of reading: VIII. Space between lines or leading, *J. Appl. Psychol.*, 16, 1932, 388-397) to be optimal. Cf. also, M. Bentley, Leading and legibility, *Psychol. Monog.*, 30, 1921, (no. 136), 48-61.

[43] The comments were received too late to affect the style of the 1928 volume as the first number of its was in type when the Washburn volume was published.

[44] Cf. D. G. Paterson and M. A. Tinker, *op. cit.*, X. Style of type face, *J. Appl. Psychol.*, 16, 1932, 608.

[45] Boring, *op. cit.*, this JOURNAL, 49, 1937, 138.

[46] M. F. Washburn, M. Bentley, K. M. Dallenbach, and E. G. Boring, Early publication of papers, this JOURNAL, 42, 1930, 1.

[47] Such as, for example, *The Psychological Record*, edited by J. R. Kantor and C. M. Louttit of Indiana University and published by the Principia Press of Bloomington, Ind., which publishes articles separately and immediately upon the payment of part of the printing costs.

(8) *The publication of two volumes a year.* The increase in size which 'prior publication' made possible, reduced for a time the lag in publication. It was not sufficient, however, to keep pace with the ever increasing number of acceptable manuscripts offered for publication. Soon again publication fell far behind acceptances. From a low of 8 months in 1930, the lag increased to a high of 21 months in 1934,[48] when the problem was once more brought before the Board. The publication of two volumes a year was then again considered. As it seemed to the Board, however, the solution of the problem did not lie in the mutiplication of publication-space. Increases in the size of journals have at best been temporary palliatives and they have sometimes cluttered the literature with inferior papers. A vigorous editorial policy could, it was thought, hold the lag to whatever latency was desired.[49] Accordingly, very strict standards regarding quality and length of papers were adopted in the fall of 1934, and the results were observed for two years. When the lag in the fall of 1936 was still found to be 18 months, the editors announced that they would "publish two volumes in 1937—one at the regular date of issue and the second (Volume 50) in November, 1937, in celebration of the JOURNAL's Golden Jubilee—and annually thereafter so long as the increase appears necessary for the prompt publication of all good, tersely written papers."[50]

The continued application of the rigorous standards adopted in 1934 has resulted in a reduction of the lag to its present level of approximately 9 months, a level that will permit articles accepted this fall to appear in the July 1938 number. A lag of this length is thought by the editors, as well as by the members of the Round Table which met in the fall of 1935 to discuss the question,[51] to be both reasonable and desirable. The JOURNAL will consequently publish but one volume during 1938. The editors are prepared, however, to issue two volumes a year if and when the lag exceeds 12 months.

PUBLICATION AND PRINTING

The JOURNAL was published from Johns Hopkins in its first two years (1¾ vols.), from Clark in the next thirty-one years (29¼ vols.), and from Cornell in the last seventeen years (19 vols.). The accompanying table lists the publishers and printers.

Publication. N. Murray was the publisher of Volume 1 and the first three numbers of Volume 2. It is not known who he was, but it is probable that he was connected with the firm that printed these numbers. E. C. Sanford, editor of Volume 2, was publisher of the fourth number of that volume and of all of Volume 3. He was succeeded as publisher in 1891 by J. H. Orpha. Who was Orpha, and what was he? According to reliable report Orpha was Hall's pseudonym. In any case, that name stands as publisher in Volumes 4-9 inclusive. Beginning with Volume 10 (1898) and running through Volume 17 (1906), Louis N. Wilson, Hall's secretary and later Librarian of Clark University, served as publisher. He was followed by Florence Chandler, of the business office of Clark University and later comptroller, who served from 1907 to 1920 (Vols. 18-31). Since 1921, the date of the

[48] Boring, *op. cit.*, this JOURNAL, 49, 1937, 138.
[49] S. W. Fernberger, Publication lag in the publications of the American Psychological Association, *Psychol. Bull.*, 33, 1936, 582.
[50] K. M. D., Editorial note, this JOURNAL, 49, 1937, 139.
[51] J. E. Anderson, Report of the Round Table on "time lag in the publication of psychological research," *Psychol. Bull.*, 32, 1935, 858.

transfer to Ithaca, N. Y., the JOURNAL has been published from the Department of Psychology, Morrill Hall, Cornell University.

PUBLISHERS AND PRINTERS OF THE JOURNAL

Vol.	Year	Publisher	Printer
1	1887	N. Murray, Baltimore, Md.	Isaac Friedenwald, Baltimore, Md.
2[1]	1889	E. C. Sanford, Worcester, Mass.	F. E. Blanchard & Co., Worcester, Mass.
4	1891	J. H. Orpha, Worcester, Mass.	
10	1898	L. H. Wilson, Worcester, Mass.	
13	1902		Commonwealth Press, Oliver B. Wood, Worcester, Mass.
18	1907	F. Chandler, Worcester, Mass.	
23	1912		Fort Orange Press, Brandow Printing Co., Albany, N. Y.
32	1921	Cornell University, Ithaca, N. Y.	
33	1922		Cayuga Press, Cornell Publications Printing Co., Ithaca, N. Y.
45	1933		George Banta Publishing Co., Wis.
50	1937		Cayuga Press, Ithaca, N. Y.

Printing. Volume 1 and the first three numbers of Volume 2 were printed by Isaac Friedenwald of Baltimore, Md. F. E. Blanchard & Co., of Worcester, Mass. printed the fourth number of Volume 2 and the next ten volumes (Vols. 3-12). Volumes 13-22 were printed by the Commonwealth Press, also of Worcester, and the succeeding ten (Vols. 23-32) by the Fort Orange Press of the Brandow Printing Co., of Albany, N. Y.

One year after the JOURNAL was transferred to Cornell, the printing was undertaken by the Cayuga Press of the Cornell Publications Printing Co., of Ithaca, N. Y. This firm printed 12 volumes (33-44) and is at present engaged in the printing of this Jubilee volume.

When the shift was made in 1933 from monotype to linotype, the work was transferred to The George Banta Publishing Co., of Menasha, Wis., where all the numbers (Vols. 45-49) have since been printed.

Copyright. The JOURNAL has been copyrighted from the beginning for the protection of its authors, all rights accruing to them. Without the copyright, neither the authors nor the JOURNAL would have any control over the material published. Permission to quote or reprint is always given gladly and freely to reputable persons, but only after the author in interest—when he can be reached—has given his consent. His wishes in the matter are final.

Reprinting. Some of the early numbers and many of the numbers published during and immediately after the War were exhausted soon after 1921. To meet the demand for back files, 26 numbers have been reprinted to date. As the work has been done by the anastatic process, the reproductions are made with photographic accuracy. Reprinting does not pay financially, yet it is justified by the service rendered in keeping the back files complete and readily available.

THE SCOPE OF THE JOURNAL

Hall's plans for the JOURNAL were, as we have seen, very ambitious. Though he founded it in the interests of experimental psychology, he

wished it to be general, to cover the entire field of scientific psychology, and to be world-wide in its scope. How far these ideals have been realized may now after fifty years be estimated by examining the breadth of its subject-matter and the geographical range of its contributors and subscribers.

Subject matter. Hall's catholic attitude toward everything that touched psychology has been fairly well maintained under the various administrations. Though largely devoted to experimental psychology, in the interest of which it was founded, the JOURNAL has published studies in all fields of scientific psychology, and it has given every new movement and school ample space for expository and critical articles. Psychoanalysis, for example, was introduced to the psychological world in its pages,[52] and *Gestalt* psychology received its first adequate and comprehensive treatment within them.[53] The JOURNAL is now and has always been general, with a strong experimental flavor, in the scope of its subject-matter.

Contributors. The range of contributors has been wide. Not only has every section of the United States been represented, but articles have been published from Austria, Australia, Canada, China, England, France, Germany, India, Japan, Russia, South Africa, and Switzerland. To this list, several countries not represented before—Holland, Italy, Jugoslavia and Poland—are added with the present volume. With a few exceptions, the articles from foreign lands have been printed in English.

Subscribers. The JOURNAL is widely circulated throughout the world. Current copies go to every state in the United States and to 43 other countries. Contributors are thus assured of a wide reading.

INDEXES

The JOURNAL has endeavored from the beginning to make its contents readily available to its readers. To that end, volume and general indexes have been published.

Volume. Every volume carries an index. The first twelve indexes do little more than list titles and authors. Beginning with Volume 13, original articles are listed in the Table of Contents, and a detailed subject-matter index, which covers everything within the pages of the volume, is followed or preceded by an author-index. References to original articles and to reviews are differentiated in Volumes 26-31 and consistently since Volume 41.

For many years (Vols. 1-40), the index listed only authors of original articles and of books reviewed (the former in small capitals). In 1929 (Vol. 41), reviewers were recognized by the inclusion of their names in the author-index. These names are differentiated by italics.

The indexes, particularly since 1921, have been carefully and painstakingly prepared. Credit for this work was not given, however, until Volume 34 (1923). In that volume and

[52] Cf. Sigmund Freud, The origin and development of psychoanalysis, this JOURNAL, 21, 1910, 181-218; Sándor Ferenczi, The psychological analysis of dreams, *idem*, 309-328; Ernest Jones, The Oedipus-complex as an explanation of Hamlet's mystery; A study in motive, *idem*, 72-113, and Freud's theory of dreams, *idem*, 283-308; C. G. Jung, The association method, *idem*, 219-269.

[53] Harry Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223.

since, the name of the indexer has been placed at the head of the section. Those recorded are: Dr. Anna K. Whitchurch, Sage Fellow in Psychology at Cornell University, Volumes 34 and 35; Professor Harry Helson of University of Kansas, now of Bryn Mawr College, Volumes 36-38; C. V. Brook of Cornell University, Volume 39; Professor J. P. Guilford of University of Nebraska, Volumes 40-48; and Dr. S. Feldman of Cornell University, Volumes 49 and 50.

General. A general index of Volumes 1-30 (1887-1919) was published in December, 1926.[54] It provides in its 200 pages an editorial history of the first 32 years of the Journal, separate lists of articles and of books reviewed, alphabetically arranged by authors, and an analytical subject-matter index (140 pages) with over 20,000 entries classified under 3000 headings and sub-headings. Since book reviews were included in the subject-matter index (entries differentiated by italics), the user of the index will find there a cross-section of the entire field of psychology during this 32-year period.

This general index was not a compilation of the volume indexes. The later were insufficiently detailed to permit doing so. Every volume was read, analyzed, and indexed anew; a task that could not have been undertaken except for the generous support and coöperation of many people. The index was prepared by 28 psychologists,[55] and standardized and edited by Dallenbach.

When this index was published the editors announced that they hoped "to issue a similar comprehensive index after the completion of every succeeding ten volumes."[56] This hope, however, was not realized, as the first index was not a financial success. Readers may recognize the value of such ventures but they do not always support them. The index planned for Volumes 31-40 had, therefore, to be given up.

The importance of general indexes to the science in general and to the authors in particular is very great. Articles that have been buried and lost sight of are resurrected and made readily available. References are brought together and placed in contexts which insures that they will not be overlooked. The advantage of this service is worth the expense and it may well be that the journal publishing general indexes profits in the long run by its increased usefulness. However this may be, a second general index, covering Volumes 31-50, is now being prepared upon the plan of the first. It will be published some time during the spring of 1938.

Portraits and Biographical Articles

From time to time, as occasion arose, portraits of psychologists have been published in the Journal. They were usually accompanied by biographical articles or obituary notes, or by studies appropriate to the occasion so commemorated. Since 1931, Boring has had the editorial responsibility of this department, which has been extended to include obituary notes of all important workers in the field of psychology.

In addition to the portraits, appearing in the frontispiece of this Volume, of the Journal's editors during the past fifty years, portraits of the following have appeared:

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[54] Madison Bentley, The general index of the first thirty volumes of The American Journal of Psychology, this Journal, 40, 1928, 523 f.

[55] For the collaborators, see page 5 of the Index.

[56] K. M. D., Index of the first thirty volumes of The American Journal of Psychology, this Journal, 38, 1927, 156.

G. Stanley Hall, Vol. 14, 1903, facing 267
G. Stanley Hall, Vol. 35, 1924, facing 313
Edmund C. Sanford, Vol. 36, 1925, facing 157
James Ward, Vol. 36, 1925, facing 449
Franz Brentano, Vol. 36, 1925, facing 469
Edward B. Titchener, Vol. 38, 1927, facing 489
Margaret F. Washburn, Vol. 39, 1927, frontispiece
Hermann Ebbinghaus, Vol. 42, 1930, facing 505
Wilhelm Wundt, Vol. 44, 1932, facing 615
Howard C. Warren, Vol. 46, 1934, facing 189
G. E. Müller, Vol. 47, 1935, facing 187
Carl Stumpf, Vol. 49, 1937, facing 173

Commemorative Issues

With the present volume, three commemorative issues have been published in the Journal. The first, dedicated to Hall "in commemoration of the twenty-fifth anniversary of his attainment of the doctorate in philosophy," was published in 1903 as numbers 3 and 4 of Volume 14, and was edited by Sanford and Titchener. This double number, 430 pages in length, includes contributions by 25 authors—from Canada, England, France, Germany, Japan, as well as the United States,—a bibliography of Hall's published writings, and a frontispiece bearing his portrait and signature.

The second commemorative issue, published in December, 1927, as Volume 39, was dedicated to Washburn "in recognition of thirty-three years of distinguished service to psychology." This *Festschrift* was edited by Dallenbach, Bentley, and Boring. It is 442 pages in length and contains 31 studies, Miss Washburn's portrait, signature, and bibliography, and a subject-matter index.

The third commemorative issue, the present volume, is published "to mark the close of the first half century" of the Journal.[87] To assist in this undertaking, every editor of every psychological periodical in the world has been invited to contribute an article. The response to this invitation was cordial and generous, and as a result the Journal commemorates its Golden Jubilee with a publication that is unique in the history of the sciences.

[87] That it was possible to issue the fiftieth volume as a separate on the fiftieth anniversary of the Journal's founding is due to a fortuitous turn of events: the loss of a year in the publication of Volumes 3 and 4 and of a second year in the publication of Volume 6; and the gain of a year by the issuance of two volumes in 1927—the regular numbers and the Washburn Commemorative Volume.

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